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THE PSYCHOLOGICAL REVIEW

A COMPARISON OF SCALING METHODS FOR AFFECTIVE JUDGMENTS

BY EDWARD N. BARNHART

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In the study of group preferences for affective stimuli, it is often desirable to be able to express the evaluations on a linear scale. This affords more exact statement of the degree of preference for each stimulus than mere average ranking in order of likings and gives a means for exact intergroup comparisons.

Several such techniques have been developed which retain the older psychophysical methods of presentation but treat the results in a way which gives a more precise picture of the various degrees of preference expressed by the group for each stimulus. Thurstone (**5**, **6**) has developed the theoretical approach and worked out the actual scaling methods for the order of merit and paired comparison methods. He has also developed an alternative method (**4**) where instead of extracting the actual proportions obtained directly, estimated proportions are calculated, and the separations between any pair of stimuli computed in terms of the frequencies with which the stimuli were placed in the rank orders. Thurstone (**2**, **3**) has also developed a 'law of comparative judgment' utilized in measuring degrees of preference of a heterogeneous group of subjects for nationalities and races, using the paired comparison method of presentation.

Hevner (**1**) has recently made an empirical study of these scaling methods using the order of merit, paired comparison and equal appearing interval techniques with a group of 370 subjects judging the excellence of handwriting samples. She

found that a linear relationship existed between the order of merit and paired comparison scale values and that both had high degrees of internal consistency.

The purpose of the present investigation was to determine whether these methods were applicable to judgments of affective stimuli, and to what degree the methods were consistent with each other. The methods used were the order of merit and the paired comparison techniques. In addition, a new method was experimentally tried, the method of single presentation, where scale values were determined from the percentage of preference expressed by the group for each stimulus when shown one at a time, each individual expressing either liking or disliking.

The subjects were forty-one girls in sophomore or junior year of college.

Materials and Methods

Two sets of stimuli were employed, ten large black and white drawings and ten smaller outline forms drawn in dark blue on white cardboard.

The drawings were 11 × 16 inches in size, drawn with black ink on white paper and mounted on cardboard. Lines, geometrical forms and other elements were combined in various arrangements. There were no forms representative of natural objects nor any attempt to depict objects. Some forms were blacked in, some had a grey wash, while many were only outlines. The compositions were in general fairly well ordered on the page but differed in their arrangements, selection of elements and neatness of arrangement.

These ten drawings were selected from a larger collection of forty-two. This larger group was presented to the subjects some time before the experiment proper and a rough method of preference indication was used. From these group preferences two drawings were taken that were highly preferred, two fairly well liked, two highly disliked, two somewhat disliked and two of indifferent status. This was to insure a heterogeneous group of drawings and to afford opportunity for preference expression.

The ten forms were simple outline geometrical forms drawn in blue crayon on 10″ × 12″ white cardboard sheets. They were in size as follows:

No. 1. Equilateral triangle—side 4 inches
2. Circle—diameter 3¼ inches
3. Isosceles triangle—base 4 inches, height 5 inches
4. Square—sides 3½ inches
5. Diamond—height 6 inches, width 4 inches
6. Five-pointed star—3 inches from center to arm tip
7. Ellipse—5 inches wide, 3½ inches high
8. Pentagon—2½ inches center to angles made by arms
9. Six-pointed star—3 inches from center to arm tip
10. Six-sided polygon—ellipse shaped—6 inches long, 3½ inches high, horizontal sides 3½ inches long.

Each form was neatly drawn and centered on the sheet.

The methods for the presentation of the specimens were as follows:

1. *Paired comparison*—Each subject was shown each pair of stimuli twice in different left-right relation, for comparative evaluation. The subject merely indicated which drawing was liked best. The pairs were presented in an order which put similar pairs as far apart in the series as possible.

2. *Order of merit*—The ten stimuli were given the subject in haphazard order. She was requested to lay them out on a large table in order of liking.

3. *Single presentation*—The stimuli were presented to the subject one at a time and she indicated whether she liked it or not. No judgments of doubt were allowed.

In all three methods the subjects were requested not to give judgments of equivalence. One or the other stimulus must be chosen as preferred.

As there were two sets of stimuli to be judged by three methods, each subject had six experimental periods. These were scheduled one week apart. No two subjects had the

same course of experiments since the order of periods was haphazard for each person. This method was to prevent any effects entering in due to all the subjects having undergone a similar series.

Scaling Techniques

The various techniques for translating group preferences to scales were as follows:

(1) *Paired comparison*—Determine for each pair of stimuli the actual proportions whereby one member was preferred to the other by the group and enter such proportions for all pairs in a table. Translate these proportions to percentage equivalents.

Using a table of deviates of the normal probability curve in terms of the area from the mean, determine the sigma distance (x/σ) of this area from the mean and enter in the table. For example, a percentage value of .866 has an area of .366 between it and the mean. The sigma value of this position is +1.1077. Each sigma value is entered twice in the table for each pair, with a positive value at the higher percentage position, indicating preferred more times, and negative at the lower.

Sum the columns algebraically and arrange the stimuli in order of increasing sigma values, from the lowest negative to the highest positive. Determine the difference between each successive pair of values and multiply by $\sqrt{2}/N$ where N is the total number of scale separations. Allocate a value of .000 to the stimulus of lowest (negative) sigma value and determine the scale values of the others by successive addition of the inter-stimulus differences.

(2) *Order of merit*—Essentially the same procedure is followed here after the table of proportions has been calculated. This is done by considering any stimulus 'higher' in an individual's rank order as preferred to those 'below' it in preference and extracting all the individual judgments. All such possible pair comparisons are tabulated for each subject and combined for the group table.

(3) *Single presentation*—Translate into percentage values the total number of times each stimulus was evaluated as 'liked' or 'disliked,' each individual being shown each specimen once for judgment. Thus specimen No. 1 was liked 82.9 per cent of its total of 41 presentations. These values were then arranged in order of increasing magnitude. Then the sigma values of these areas from the mean were found from the table. As specimen No. 1 had a percentage preference of 82.9 per cent, its sigma score was found to be + .9502. Then the differences between the successive specimen's sigma scores were computed, the least liked specimen given a value of .000, and the scale values of the other specimens computed by successive addition.

Results

The following tables indicate the results of scaling the preferences of the group for the forms and drawings by the above methods. The specimen with value of .000 is the one least liked, the larger the scale value the greater the preference indicated for it.

Drawings

Specimen	Paired Comp.	Specimen	Order of Merit	Specimen	Single Pres.
No. 2	.000	No. 2	.000	No. 2	.000
3	.201	10	.244	6	.215
5	.475	5	.332	10	.305
10	.508	3	.334	3	.389
6	.553	6	.382	5	.620
4	1.327	4	1.526	4	1.195
7	1.498	7	1.538	8	1.318
8	1.800	8	1.623	7	1.318
1	2.085	9	2.194	9	2.025
9	2.254	1	2.527	1	2.115

The scale-values of each method were correlated by the product-moment method with the values of the other methods. The following values were obtained:

1. Order of merit with single presentation..... .983
2. Paired comparison with single presentation..... .968
3. Order of merit with paired comparison..... .977

FORMS

Specimen	Paired Comp.	Specimen	Order of Merit	Specimen	Single Pres.
No. 7	.000	No. 7	.000	No. 7	.000
4	.781	8	.500	4	.809
2	.957	10	.565	10	.874
10	1.002	4	.636	8	.874
8	1.114	2	.790	2	.874
1	1.346	1	1.136	3	1.002
9	1.391	3	1.362	9	1.214
3	1.435	9	1.502	1	1.367
5	1.872	5	1.803	6	1.454
6	1.978	6	1.989	5	1.888

The inter-scale-value correlations were as follows:

1. Order of merit with single presentation..... .892
2. Paired comparison with single presentation..... .954
3. Order of merit with paired comparison..... .948

PAIRED COMPARISON CONSISTENCY

To determine how consistent the judgments of each subject were throughout the course of the paired comparison experiments a study was made of the judgments. The paired comparison form of the constant method has long been held to be the ideal technique for determining preferences since all of the stimuli serve as standards whereby the others are evaluated.

The consistency of each subject's judgments was computed for both the form and drawing presentations, with the use of the tetra-choric correlation technique. Each of the ninety pairs of judgments in a subject's paired comparison experimental period was examined for reversals and the correlation computed for each subject. The higher the correlation, in general, the fewer reversals were present. For the group the average reliability for the forms was .913 and for the drawings .914 with a small range of values.

These high values indicate that there was very little change of preference during the course of the paired comparison presentations, as indicated by reversals of judgment. The group as a whole stood by its tastes without much change.

Method of Estimating Proportions

An examination was made of Thurstone's alternative method for scaling rank order preferences (4). Instead of extracting all possible pair comparisons from each subject's rank order of preferences and combining for the group (as described in the section on Scaling Techniques), a long procedure if more than ten or twelve specimens are used, it is possible to employ a shorter alternative method by computing the frequency for the group with which each of the n specimens is placed in each one of the n rank orders. From this table another table is prepared showing the percentage proportions of all subjects who placed each specimen in each of the rank orders. The frequencies are merely changed into percentage values. Then for each specimen a two-column strip is computed: (1) indicating for all n ranks the proportion of subjects placing the specimen in question in each rank, and (2) indicating the percentage of subjects placing the specimen in ranks higher than any specific rank. Thus a strip for a specimen No. 1 might be as follows:

Rank	Proportion of Subjects Placing in Each Rank, (P_{1k})	Proportion of Subjects Placing Specimen in Ranks Higher than any Specific Rank ($P_{1>k}$)
1	.024	.976
2	.049	.927
3	.098, etc.	.829, etc.

Thus the table reads that .024 per cent of the subjects placed specimen No. 1 in rank 1, .049 per cent placed it in rank 2, and so forth. Thurstone designates these proportions as 'p_{1k}' where k is any rank and 1, 2, $\cdots$ are specimen numbers. Column three indicates that .976 per cent of the subjects placed specimen No. 1 in ranks higher than rank 1, .927 per cent placed specimen No. 1 in ranks higher than rank 2, and so forth. These proportions are designated '$p_{1>k}$.'

For each of the $\frac{1}{2}n(n-1)$ possible pairs of stimuli calculations are carried out according to the formula developed by Thurstone:

$$\Sigma(p_{1k} \cdot p_{2>k}) + \tfrac{1}{2}\Sigma(p_{1k} \cdot p_{2k}) = p_{2>1},$$

or the proportion of subjects placing specimen 2 over specimen

1 in preference. This gives for every possible pair of stimuli, the estimated proportions of subjects who 'judged' one specimen as preferred to the other. These proportions in turn are entered in a table and the scale values calculated as described above in the order of merit method.

This method was carried out on the group judgments with the forms. The resulting scale values were as follows:

Specimen	Scale Value
No. 7	.000
8	.491
10	.553
4	.621
2	.732
1	1.160
3	1.391
9	1.505
5	1.905
6	2.012

These scale values, correlated with the scale values obtained by the method of actual proportions, gave a value of .999. The agreement between the two scales is almost unity, the same rank order being obtained but with slight scale differences.

The discrepancies between the estimated and the actual proportions were computed in the form 'actual proportions minus computed.' The average discrepancy was $-.005$ with a range of values from .062 to $-.080$. This small average dispersion indicates that the method of estimating proportions gives almost exactly the same proportions as the actual counting procedure does. In the study mentioned above, Thurstone (4) obtained an average dispersion of .0078 regardless of sign. Inasmuch as both these average dispersions were less than one per cent it would seem indicated that for practical purposes the equation and procedure are justified as a method of obtaining proportions when the labor of actual determination is too great.

Conclusions

1. The scaling techniques, though based on different methods of stimuli presentation, gave approximately the same scale values in the case of two different types of materials.

2. Since the scale values of the method of single presentation correlated so highly with the values from the method of paired comparison, which latter method was found to have a high degree of consistency with average reliabilities over .91, it appears to be justified as a reliable technique for determining group preferences. The single presentation technique is much simpler since it requires only the determination of the per cent of 'preferences' expressed by a group of subjects for stimuli presented individually.

3. The method of estimating proportions is shown to be justified since the resulting scale values correlated almost unity with the scale values derived from the actual proportions, and discrepancies were small.

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THE COMPLETION HYPOTHESIS AND REINFORCEMENT

BY RALPH K. WHITE

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I. The Problem

Hull's goal-gradient hypothesis attributes the reinforcement of S–R connections to a subsequent or concomitant 'goal situation,' but he has not, as yet, formulated any general hypothesis as to what kinds of situations are goals and what kinds are not (**6**, 26; **8**, 424). The uncertainty is not serious, of course, when hungry rats are run through a maze with food as a reward, or when sexually excited males are run through a maze with a receptive female as a reward. In such cases the goal or reward is so consistently found to possess a reinforcing function, and it is so closely related to basic biological values, that the need for a more adequate explanation may not be very keenly felt. The problem does become of obvious importance, however, when reinforcement is brought about by situations which do not usually, or did not originally, possess the power to reinforce. There can be no doubt that this often occurs. Williams, for instance, found that rats 'learned' very rapidly to enter maze-alleys leading to a certain empty discrimination-box (**22**). There was no food or other biologically useful object in the box, and certainly there is no inherent characteristic of the organism which causes a discrimination-box as such to have a clear-cut reward value; but speaking operationally, in terms of observed reinforcement, this particular box unquestionably functioned as a reward. In human behavior the phenomenon is even more conspicuous. Thorndike, for instance, has repeatedly obtained objective and quantitative evidence that under certain conditions there is a reward value in the announcement of 'Right' instead of 'Wrong' (**19**, 276–299). That is to say, there was consistent evidence of a tendency to the reinforce-

ment of the immediately preceding S–R connection. If the subjects had been ignorant of the English language, or had been given a contrary set of instructions, it is fairly certain that the reinforcement would not have occurred. Thorndike's announcement of 'Right' is apparently analogous, then, to Williams' empty discrimination-box. In both cases reinforcement was brought about by a situation which would not, because of any inherent characteristic of the organism, possess for that organism a reward value. How can such a thing take place? Is there any generalization by means of which it might be predicted that a hitherto neutral situation will function as a reinforcing agent?

One simple answer to this question is accepted as almost axiomatic by many present-day psychologists who hold in other respects the most diverse theoretical points of view. It is accepted, for example, not only by McDougall (**12**, 15), Tolman (**20**, 28), and Lewin (**11**, 58, 79), but also by Thorndike (**19**, 396). Although they express their answer in very different terminologies, all of these men seem to agree that *a goal or reward situation exists* (*i.e., reinforcement occurs*) *whenever a 'want is satisfied' or a 'tension is released.'* In answer to the question "What determines the goals of a particular organism at a particular moment of time?," these men would probably agree that the goals are determined by the 'wants,' 'motives,' or 'tensions' which are active within the organism at that time. In Williams' experiment, according to this hypothesis, reinforcement would be expected to occur if for any reason the rats 'wanted' the empty discrimination-box; and in Thorndike's experiments, reinforcement would occur if his human subjects 'wanted' to hear the experimenter say 'Right' instead of 'Wrong.' For convenience this can be called *the satisfaction hypothesis.*

Surely the facts of everyday observation and of experimentation (see especially **20**, 39–68) are adequate evidence for the validity of this hypothesis, as far as it goes. It is now beyond any reasonable doubt that *some* pre-existing internal process or condition—which can then be called a 'want,' a 'motive,' a 'tension,' or something else—is of

primary importance in determining what situations will produce reinforcement and what situations will not.

The problem still remains, however: What really happens when a 'want is satisfied' or a 'tension is released'? What conceivable physical mechanism underlies this easily-recognized subjective phenomenon? Or, in the absence of direct physiological data, is it possible to set up a physical hypothesis which can be even partially and indirectly verified by the experimental facts?

One such hypothesis appears to be available if the satisfaction hypothesis is translated into Hull's terminology, and if the implications of that terminology are rigorously adhered to. The purpose of the following pages is to make the translation and to summarize the relevant data. The translation seems worth while, partly because it produces an interesting hypothesis, and also partly because it makes possible, at one important point, an integration of Hull's theoretical constructs with those of McDougall, Tolman, Lewin, Thorndike, and others.

II. The Translation [1]

Hull has suggested that what is usually called a 'want,' 'motive' or 'tension' can be conceived more concretely as a particular sort of anticipatory reaction. (By 'anticipatory reaction' he means simply a reaction which occurs as an immediate response to a situation which originally preceded it by an appreciably longer interval of time. This is an objective use of the term, and does not imply conscious anticipation or foresight.) He presents reasons for believing that such a response would tend to become a conditioned stimulus to adaptive responses—'adaptive' in the sense that they have previously preceded some goal situation and hence, more often than not, are objectively useful in 'attaining' that goal. In other words, he has suggested a process by which an actual physical response might come to possess the behavior-directing function of a 'want' or 'tension.'

[1] The remainder of this article presupposes an understanding of Hull's postulates, and of the evidence upon which they are based, as represented in his recent theoretical articles (3, 4, 5, 6). Of these (5) is the most relevant.

So far, the task of translation looks easy. It seems as though all that is needed is to substitute the term 'anticipatory reaction' for the word 'want.' But the question immediately arises: How can we express, in Hull's terms, the *satisfaction* of a want? Is there in his system any purely objective concept which can be postulated as the internal mechanism responsible for the observed facts of behavior-reinforcement?

There appears to be, in Hull's system, only one concept that is available for the purpose. It is the *completion* of an incomplete, incipient, 'fractional' anticipatory reaction. Hull has suggested reasons for believing that, as a rule, only a small part or fraction of a complex response can become anticipatory. If the whole response came forward it would conflict with other, non-anticipatory responses (**5**, 493). Salivation, for instance, is only part of the complete biting-tasting-chewing-salivating-swallowing-digesting response which becomes possible when a hungry and already salivating animal obtains food. The fractional anticipatory reaction is an incipient response or an 'activity in progress' in the literal sense that it is a specific physical act which has been started but not yet completed. The goal-situation makes possible the rounding-out of a coördinated activity-pattern, or the finishing of a complex act in the same manner in which it has been finished on previous occasions. If, then, 'completion' means the transition from an incipient reaction to the complete reaction of which it was previously a part (in Hull's symbolism, the transition from r_G to R_G), our translation of the satisfaction hypothesis can be expressed as follows: *The completion of a fractional anticipatory reaction tends to reinforce recent and concomitant S–R connections.* For convenience this can be called *the completion hypothesis.* It is a simple translation of the satisfaction hypothesis, but, because of the physical meaning which Hull has given to its central term, the fractional anticipatory reaction, it has implications which are new. It implies that a 'want' or 'tension' is literally some kind of physical response [2] which is anticipatory in Hull's objective

[2] This allows the greatest possible leeway to the neural physiologist in determining *what* kind of physical response a 'want' or 'tension' really is. Hull has suggested that

sense of the word. It implies Hull's hypothesis that the anticipatory reaction is usually only a fractional part of the complete response from which it is derived.[3] It implies, finally, that the completion of this incomplete response is the physical event corresponding to the attainment of a 'goal' or 'reward,' and is somehow responsible for reinforcement.[4]

III. The Evidence

The validity of the completion hypothesis depends upon the validity of one of Hull's theoretical constructs (the fractional anticipatory reaction functioning as a behavior-directing stimulus), and this has not yet been widely accepted

in some cases it might be only a 'neural vestige' (4, footnote, p. 518). Recent work by Sherrington (16), Rosenblueth (15) and others (1, 186–90) supports the hypothesis that every neural response is effected by a preliminary accumulation of some excitatory state or substance, which must reach a certain liminal quantity before the response takes place. This accumulation is, in a sense, a condition of disequilibrium or neural 'tension,' and equilibrium is not ordinarily restored until the excess is depleted in the process of initiating one or more nerve impulses (1, 188). If the limen were raised, or if the excitation were to be generated faster than it could be taken care of, it is logical to suppose that a condition of relatively acute disequilibrium might be maintained for some length of time. Lashley suggests that the concept of equilibrium may also be useful in speaking of functional brain fields rather than of a single neuron (9, 23; 10, 474). It is therefore at least conceivable that the physical basis of a 'want' is a heightened neural excitation or potential of some sort, which for some reason is bottled up and tends to persist until new environmental conditions somehow make possible a 'release of tension' or 'discharge of excitation' through a different sort of neural or neuromuscular activity. Willoughby has found that a rather extensive range of psychoanalytic and experimental data can be unified in terms of a very similar postulate (21, 461–471). Such an interpretation could be combined with Hull's hypothesis, if and when the neural disequilibrium or excitation is anticipatory in Hull's sense of the word.

[3] The neural excitation or disequilibrium which initiates any response can be looked upon as a fractional part of that response. Surely it is 'incomplete' if the excitation is bottled up until the pressure becomes so great that the dam breaks (this seems to be what happens in the sex act), or until a particular complex of environmental stimuli touches off the accumulated energy and the complete response occurs (this seems to be what happens when a hungry animal eats food or when a thirsty animal drinks water).

[4] Hull has almost, but not quite, taken this step. He has called attention to the resemblance between the completion of an anticipatory reaction and the fulfilment of an expectation (5, 505), but he has not postulated a causal relationship between this event and reinforcement, nor clearly differentiated between wants and expectations. There is a knotty problem here. It is possible that the completion hypothesis holds only for those anticipatory reactions which can be characterized as wants, and not for those which can be characterized as expectations, especially anxious or apprehensive expectations, such as the expectation of an electric shock.

or supported by crucial experiments. Until the methods of neurophysiological research have become much more refined it will be impossible to test directly the hypothesis that a 'want' is a condition of anticipatory excitation or disequilibrium within the nervous system. On the other hand, there is a rather extensive assortment of indirect evidence. Since most of it is covered in the theoretical writings of Hull, Tolman, and Lewin, and since it would be fully understood, in any case, only by those who are already familiar with their work, it will be summarized here in the briefest possible manner:

A. Facts Emphasized by Hull

1. Trial-and-error learning. (See Hull's deduction of it from 'conditioned response' principles (**3**, 244–248); and note that his use of the term 'reinforcement' in this context is equivalent to the process which Thorndike would call 'confirmation' or 'strengthening,' and which he would attribute to a 'satisfying state of affairs.')

2. Transfer of training to non-practiced parts of a habit-family hierarchy in which an anticipatory reaction can be postulated as a common stimulus (**7**, 41–48).

3. The general anticipatory tendency of the conditioned response (**18**; **17**; **8**, 433).

4. The coincidence that certain 'consummatory' reactions (such as eating and sex reactions), which rather consistently function as rewards or reinforcing agents, also have an especially marked tendency to become anticipatory (**5**, 488, 491, 493). The completion hypothesis suggests a particular sort of causal relationship here, the reward value being interpreted as a consequence of an inherent tendency to become anticipatory in an incipient form, rather than *vice versa*. This constitutes a new hypothesis as to the nature of innate appetities or 'instincts.' For example, the innate food-seeking tendency might be attributed to the reinforcement caused by completion of incipient eating responses: *e.g.*, anticipatory salivation, anticipatory tonus in the chewing and swallowing muscles, or perhaps simply anticipatory excitation in neural centers involved in the act of eating.

5. The fact that the conditioned-response principles of experimental extinction and spontaneous recovery apply to at least certain kinds of motivation (**22, 8, 13**).

B. Facts Emphasized by Tolman

1. The tendency for a 'means-object' to function as a 'goal-object' (**22; 20; 28,** 143–180). Since Hull has suggested reasons why a 'subgoal' response should have an especially strong tendency to become anticipatory (**7,** 146), the addition of the completion hypothesis makes it possible for his system to account for the phenomenon, at least in part.

2. The facts of latent learning and of insight (**20,** 48–53, 164–180, 339–370), which can be interpreted in terms of the tendency for means-objects to function as goal-objects.

3. The great importance in human behavior of 'second-order drives,' such as the desire for social approval, which can be interpreted as ultimately based upon the tendency for means-objects to function as goal-objects (**20,** 290–294). It is reasonable to suppose that the desire for approval is involved in Thorndike's experiments, so that his evidence on the reinforcing value of the announcement of 'Right' can be interpreted in terms of the same mechanism.

C. Facts Emphasized by Lewin and Others

1. The tendency for an activity-in-progress (which, according to Hull's system, should generate many incomplete anticipatory reactions) to function as a motive (**23, 24, 14**).

2. The facts of perceptual 'closure' (**2,** 97–98, 184–185) which appear analogous to the postulated completion mechanism.

Summary

1. Hull's goal-gradient hypothesis attributes reinforcement to a goal-situation, but does not specify what kinds of situations are goals.

2. The 'satisfaction hypothesis,' accepted as almost axiomatic by many present-day psychologists, asserts that a goal or reward situation exists (*i.e.*, reinforcement occurs) whenever a 'want is satisfied' or a 'tension is released.'

3. There is much evidence for the validity of this hypothesis, as far as it goes, but the question still remains: What conceivable physical mechanism underlies the 'satisfaction of a want'?

4. A translation of the satisfaction hypothesis into Hull's terminology suggests the following hypothesis: The completion of a fractional anticipatory reaction tends to reinforce recent and concomitant S–R connections.

5. There is no direct evidence for this postulate, but there is a considerable amount of indirect evidence.

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A SCALE FOR THE MEASUREMENT OF A PSYCHOLOGICAL MAGNITUDE: LOUDNESS

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A scale adequate to the measurement of the subjective magnitude of sensation has long been sought by psychophysicists. Scales, such as those obtained by summating difference limens, have been proposed, but they have turned out to be scales of intensive magnitude, *i.e.*, scales which enable us to tell whether one sensation is greater than another without, however, permitting us to say *how much* greater.[1] In other words, it has not been possible to apply to these scales numbers for which the usual laws of mathematics could be shown to hold when the numbers were given operational meaning, *i.e.*, identified with concrete operations performed upon the organism.

I

The Problem of the Scale

We devise scales for the purpose of facilitating the description of natural phenomena in terms of functional relationships expressed, if possible, by the symbols of conventional mathematics. Consequently, it is desirable to assign numbers in each scale which not only denote the order within the scale (for which the letters of the alphabet would serve well enough), but also designate the relative magnitudes of the phenomena to which the scale is applied. When this is done, the scale numbers can be manipulated in accordance with arithmetical laws in order to determine additional relationships such as the sum of two magnitudes, the relative separation of two pairs of magnitudes, etc. However, the outcome of the purely

[1] The operational basis of psychological measurement has been treated by D. McGregor, Scientific measurement and psychology, Psychol. Rev., 1935, 42, 246–266.

formal [2] (mathematical) manipulation of the scale numbers has no empirical significance unless the manipulations and their results can be identified with some concrete operations. Thus, in the case of a scale of length, 3 cm. plus 3 cm. equal 6 cm. both formally and empirically, because the operation of adding lengths can be carried out and the formal result verified empirically. In the case of electrical inductances 3 henries plus 3 henries equal 6 henries, but here the physical process of adding the two inductances in order to verify the relationship is very different from that of adding two lengths. A coil of wire whose inductance is 3 henries does not become one of 6 henries when we simply double the length of the wire. Other criteria have to be chosen to define the scale used in the measurement of inductance. Similarly, for every scale obeying the laws of addition, the physical process represented consists of a different set of operations.

Now, in the case of sensation what we want is a scale for the measurement of some aspect of the response of a living organism to a certain class of stimuli. Two conditions should be satisfied. First, the scale numbers should be applied to the attribute of sensation in such a way as to make the scale one of true numerical magnitude, which means simply that if the numbers are manipulated according to the rules of arithmetic, the result (and the manipulations) correspond to a set of physical operations. Secondly, although at the outset we could conceivably choose any one of several sets of operations as defining the scale,[3] that set will ultimately prove to be most satisfactory for a subjective scale when it leads to scale numbers bearing a reasonable relationship to the experience of the observer. Thus, the scale would be satisfactory if the magnitude of the attribute of sensation to which the number 10 is assigned should appear to be half as great to the experiencing individual as that to which the number 20 is given, and twice as great as the magnitude to which the number 5

[2] For the distinction between what is here meant by formal and empirical statements see S. S. Stevens, Psychology, the propaedeutic science, *Philos. Sci.*, 1936, 3, 90-103.

[3] For a recent discussion of three of these sets of operations see C. H. Graham, Psychophysics and behavior, *J. Gen. Psychol.*, 1934, 10, 299-310.

is given. With such a scale the operation of addition consists of changing the stimulus until the observer gives a particular response which indicates that a given relation of magnitudes has been achieved. In other words, a subjective scale is a scale of response, and the response of the observer who says "this is half as loud as that" is one which, for the purpose of erecting a subjective scale, can be accepted at its face value.

A scale, then, which would enable us to designate the *numerical* as well as the *intensive* magnitude of an attribute of sensation can be constructed according to the criterion that, having assigned a particular number N to a given magnitude, the number $N/2$ shall be assigned to the magnitude which appears half as great to the experiencing individual. Obviously, in the application of this criterion we are limited by our ability to devise operations for the determination of fractional magnitudes of sensation.

We do not measure the magnitude of a sensation, but only of a particular dimension or aspect of sensation within a single sensory modality. Thus a scale for each of the four attributes [4] of auditory sensation could conceivably be devised, and the experience attendant upon stimulation by a pure tone could be described by four numbers, one for its position on each of the four subjective scales. Any two numbers will, of course, uniquely determine the tone.

In proceeding to the construction of a scale for an auditory attribute, loudness let us say, we are confronted with the possibility of measuring the attribute as a function of either one of the two dimensions of the stimulus, frequency and intensity, since each attribute is a function of both dimensions.[5] However, in the case of loudness it is obviously more satisfactory to hold frequency constant and determine loudness as a function of intensity, since this function is single valued, and variation is possible over the entire range of the attribute. (At a fixed level of intensity loudness is not a single valued function of frequency.)

[4] For a discussion of the criteria of auditory attributes see S. S. Stevens, The operational definition of psychological concepts, PSYCHOL. REV., 1935, 42, 517-527.

[5] S. S. Stevens, The attributes of tones, *Proc. Nat. Acad. Sci.*, 1934, 20, 457-459.

Having decided, then, upon the function which we wish to establish, it remains for us to devise procedures that will yield results satisfying the criteria which define the scale. Loudness is a name which we give to a class of discriminatory responses on the part of an organism under certain restricted conditions of 'set' and stimulation, and it is these responses which must provide the basic operations that we seek. In other words, the subjective judgments (responses) of the observer must provide the ultimate test of the validity of the numbers on the scale as representative of degrees of loudness. The utilization of the observer's discriminations in this way presupposes, of course, that he is capable of making valid judgments of the numerical ratio of one impression to another. How he acquired this ability is irrelevant to the present problem, providing he *has* the ability.

If the observer is able to make what we can accept as valid judgments of the ratio of two loudnesses, the problem becomes simply one of finding the particular ratio and the experimental conditions which will make the judgments most reliable. In the case of loudness the simple ratio 1 to 2 has been determined with good consistency by several investigators, and four general methods have been used to establish it.[6]

1. The observer is required to make a direct estimate by varying the intensity of one tone until it sounds half as loud as another. Several variations of this procedure are possible. This method is necessarily the most basic (although not necessarily the most reliable) one in view of the criteria previously laid down regarding the nature of the loudness scale, and the other methods are valid only in so far as they offer alternative ways of getting the same results.

2. The second method makes use of the fact that the two ears are connected in such a way that a tone introduced into one ear sounds half as loud as the same tone introduced into both ears. (The ears are apparently connected in a manner analogous to voltaic cells in series in that summation occurs, whereas the eyes suggest somewhat a parallel connection.)

[6] B. G. Churcher, A loudness scale for industrial noise measurements, *J. Acoust. Soc. Amer.*, 1935, 6, 216–226.

The procedure then is to have the observer adjust the intensity of a tone in one ear until it sounds as loud as a given tone in both ears. The validity of the results of this method are checked by their agreement with those of the first method.

3. Since differential sensitivity to intensity is a function of the intensity level, the loudness of a tone, heard binaurally and yielding the same difference limen as a tone heard monaurally, might be considered half as loud as the latter provided again that these results agree with those gotten by the first method.

4. Two tones of equal loudness which are sufficiently separated in frequency so as not to stimulate overlapping areas on the basilar membrane ought to yield, when presented together, a loudness twice as great as either one alone. By equating a third tone first to one and then to both, the desired ratio of loudnesses should be obtained.

II

The Loudness Scale

The results of recent studies which utilized these methods have been reviewed and summarized by Churcher,[7] who concludes that sufficiently good agreement is shown to warrant the construction of a loudness scale based upon the weighted average of the results of several experiments. Thus the curve labeled '*loudness*' in Fig. 1 he constructed by assigning arbitrarily the number 100 to the loudness of a tone (1000 cycles) 100 db above the standard reference threshold of 0.0002 bars. Consequently the number 50 means an intensity which sounds half as loud as 100 and the number 10 an intensity which sounds one tenth as loud, etc. This curve expresses completely, therefore, the relation between a dimension of the stimulus and an attribute of auditory sensation.

On the physiological side, H. Davis and the author have measured the magnitude of the electrical potentials generated in the cochleas of guinea pigs in response to stimulation by a 1000-cycle tone at various intensity levels. These potentials (obtained from three animals) are plotted as circles in Fig. 1.

[7] B. G. Churcher, *op. cit.*

The values were multiplied by a factor to make the value at 80 db above threshold correspond to the loudness curve at

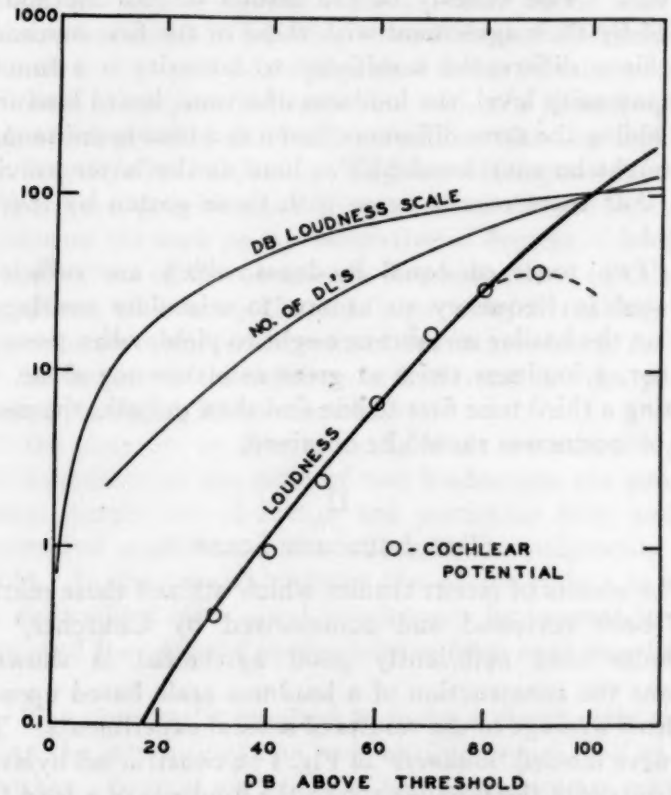

FIG. 1. The relation of loudness and number of DL's to the intensity of the stimulus at 1000 cycles. The curve for loudness was obtained from a consideration of the results of several experiments reviewed by Churcher. The circles represent the magnitude of the electrical response generated in the cochleas of guinea pigs and recorded on a cathode-ray oscillograph. The values were multiplied by a constant to make them coincide with the loudness curve at 80 db above threshold. Above that intensity the potentials decrease with intensity due to an overload effect. The curve for the number of DL's above threshold was adjusted to correspond to the loudness curve at 100 db and therefore the ordinates for this curve are relative values only. The db loudness scale, which is simply the locus of points for which the ordinate value is equal to the abscissa value, is to illustrate the divergence between the true loudness scale and the decibel scale by which we usually measure the stimulus.

that intensity in order to facilitate the comparison of the two functions. The close agreement suggests not only that it is possible to establish a valid scale for loudness, but also

that there exists in the sense-organ a directly measurable process whose magnitude, to a first approximation, is the same function of intensity as is the loudness scale.[8] It should be noted in this connection that, since the loudness curve of Fig. 1 represents averaged data from several individuals, it has the 'properties of an average.' Consequently it may not be as useful for deducing the nature of the mechanism of the response to loudness as would be the case if it represented homogeneous data from a single ear, for which the properties (variability, etc.) were known.

Since the loudness scale will probably be utilized extensively by psychophysiologists and acoustical engineers, it seems appropriate to select and name a fundamental unit of loudness. The loudness of a 1000-cycle tone 40 db above threshold, listened to with both ears, recommends itself as the logical unit, because 1000 cycles has been selected as the reference-frequency for loudness comparisons leading to the determination of *loudness level*, and the loudness level of 40 db above threshold has been proposed as the reference-level for determining the *pitch* of a tone.[9] Incidentally, on Churcher's scale (Fig. 1) the value 1 corresponds almost exactly to an intensity of 40 db above threshold. Such a unit should prove to be of the right order of magnitude for general usefulness, since it is only about one-third of one per cent of the maximal loudness the ear can support. As we shall see later this unit corresponds in order of magnitude to the differential thresholds of moderately intense tones of the musical scale. As a name [10] for the unit the word *sone* is proposed.

The Measurement of Difference Limens

Armed with a scale for the measurement of subjective magnitudes, it now becomes possible to answer the question

[8] A more complete discussion of the physiological correlates of loudness is scheduled to appear in the *J. Acoust. Soc. Amer.*, July, 1936.

[9] H. Fletcher, Loudness, pitch and the timbre of musical tones and their relation to the intensity, the frequency and the overtone structure, *J. Acoust. Soc. Amer.*, 1934, 6, 59–69.

[10] The name *phone* was proposed by the author at the meeting of the Acoustical Society of America, Harvard University, December, 1935, but was found to be objectionable because in Germany *phon* is used to designate the equivalent of a decibel. There appear to be no such objections to the word *sone*, however.

which has agitated psychologists since the days of Fechner regarding the subjective size of a just noticeable difference (DL). Fechner assumed that all DL's are subjectively equal and proceeded forthwith to integrate them in an effort to determine the magnitude of sensation. However, it has been found that summating the same number of DL's for two tones of different frequency does not yield equal loudnesses.[11] Work on equal sense-distances also failed to confirm Fechner's assumption, but suggested that the DL's at high intensities are subjectively larger than those at low intensities, although it could not be said how much larger.[12] It should be noted that we are not here concerned with the constancy of the Weber-fraction, which was *another* of Fechner's assumptions, but only with the problem of subjective magnitude, *i.e.*, the ability of an added just noticeable difference to contribute an increment to the total subjective effect of the stimulus.

If Fechner's first assumption were correct, the summated DL's would yield a function proportional to the loudness function shown in Fig. 1. However, a determination of the number of DL's contained in a 1000 cycle tone as a function of intensity gives the relationship labeled 'No. of DL's' in Fig. 1. This curve was obtained from an integration of Riesz's [13] measurements of differential sensitivity. Riesz's values were multiplied by a constant in order to make the two curves correspond at 100 db and thereby reveal the wide divergence between the two functions. The curve marked 'db loudness scale' has been inserted in an effort to correct the impression that the decibel scale is a fair measure of loudness. If it were, this curve would correspond to the loudness curve.

[11] E. B. Newman, The validity of the just noticeable difference as a unit of psychological magnitude, *Trans. Kansas Acad. Sci.*, 1933, 36, 172–175.

[12] E. B. Titchener, A text-book of psychology, New York, 1910, p. 218. Titchener, however, rejected the implication that the DL's are not equal. He argued that the stimulus-error might have invalidated the measurements of equal sense-distances. A final answer to the question of the relation of summated equal sense-distances to the loudness scale obtained by the procedure outlined in the present paper cannot be given until more adequate measurements have been made.

[13] R. R. Riesz, Loudness and the minimum perceptible increment in intensity, *J. Acoust. Soc. Amer.*, 1933, 4, 211–216.

Now, Fig. 1 shows the relationship between loudness and intensity, and between the number of DL's and intensity. We can proceed, therefore, to determine the relation between the number of DL's and loudness and thereby to measure the one in terms of the other, just as we measure the ratio of one distance to another by determining the relation of each separately to a third scale. The relation turns out to be very nearly exponential as is shown by the fact that, by plotting the two functions in logarithmic coördinates, straight lines are obtained (see Fig. 2). In order to plot these functions for frequencies other than 1000 cycles it was necessary to put the data through the following series of transformations. From the data of Fletcher and Munson [14] the loudness level of the tone was determined as a function of intensity level, and the loudness corresponding to each loudness level was obtained from the loudness curve of Fig. 1. Thence the relation of loudness to intensity level was obtainable. Next the relation of number of DL's to sensation level (db above threshold) was obtained from Riesz's data, and then, by adding the threshold intensity level, the number of DL's as a function of intensity level was found. Hence, knowing the relation of both loudness and number of DL's to the scale of intensity level, we could determine their relation to each other. This computation was made for tones of 200, 4000 and 7000 cycles—the tones for which complete data were available.

In fitting lines to the data in Fig. 2 the best fitting straight line (visually determined) was passed through the points for 200 cycles, and then lines parallel to it were drawn through the other sets. Then, if the lines are taken as defining the relationship between loudness and the number of DL's above threshold, the equation shown in Fig. 2 can be written. The constant K can be determined from the intercepts of the lines with the loudness axis. The exponent is the same for all frequencies because the slopes of the lines are the same. Of course, since the data for the higher frequencies could best be

[14] H. Fletcher and W. A. Munson, Loudness, its definition, measurement and calculation, *J. Acoust. Soc. Amer.*, 1933, 5, 82-108.

fitted by curves slightly concave downward, the exponent is not strictly constant, but the data probably do not warrant

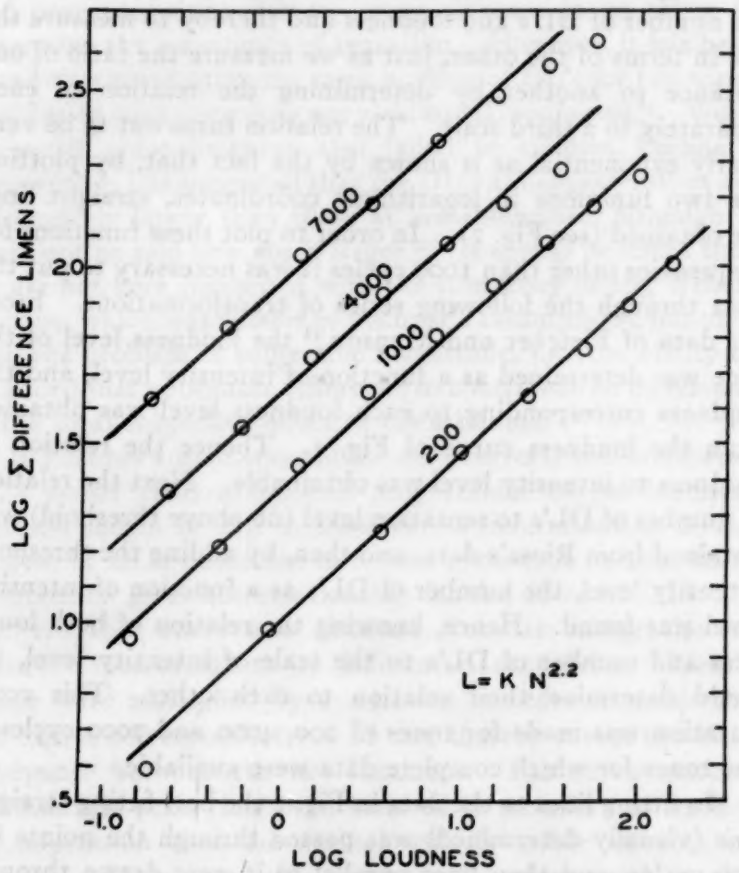

FIG. 2. The relation of loudness to number of DL's for tones of four different frequencies. The points for 7000 cycles have been shifted 0.5 logarithmic units upward on the ordinate scale in order to facilitate plotting, since the function for 7000 cycles lies between those for 1000 and 4000 cycles. L represents loudness, N the number of DL's above threshold and K is a constant determined by the intercepts of the lines on the axis of log loudness at zero value of the ordinate. The values for K are: .0070, .00112, .00028, .00070 for 200, 1000, 4000, and 7000 cycles respectively.

more precision in the determination of the exponent than has been used here. On Fechner's assumption the exponent would be unity.

Now, to measure the size in sones of the first DL above threshold we may set $N = 1$, so that the size becomes equal to the value of K. In Fig. 3 the magnitude of the first DL is plotted as function of the frequency of the tone. The general relationship would be the same for the Nth DL. The DL's for tones at about 3000 cycles are all subjectively smaller than those at other frequencies.

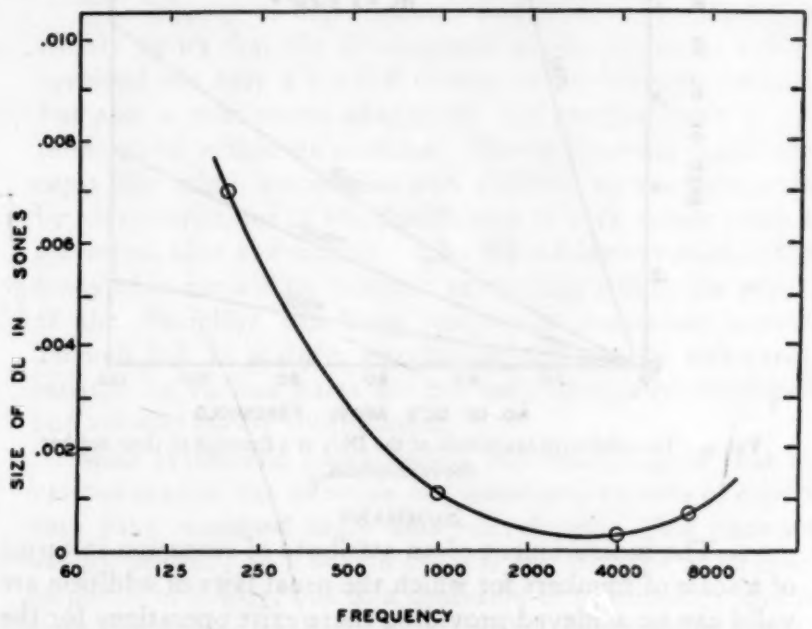

FIG. 3. The variation of the subjective magnitude of the first DL above threshold as a function of frequency. The circles are for the four frequencies for which values were computed. The curve was fitted visually.

Not only does the subjective magnitude of a DL depend upon the frequency of a tone, but it varies also as a function of the number of the DL above threshold. This relationship is shown in Fig. 4. The equation in Fig. 4 relating the size of a DL to its number is obtained by differentiating the equation in Fig. 2. The vast disparity between the subjective magnitudes of different DL's for a tone of given frequency is astonishing in view of the original assumption by which they were considered equal. Their integration for the purpose of

obtaining a reasonable numerical scale for the measurement of the magnitude of 'sensation' is obviously not valid.

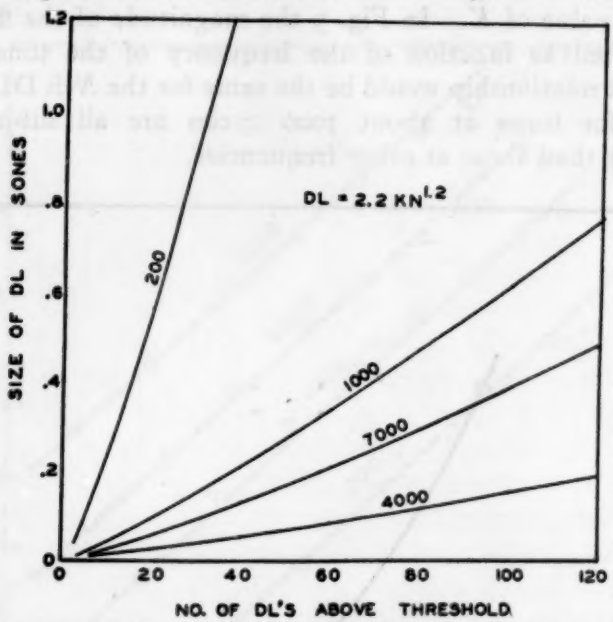

FIG. 4. The subjective magnitude of the DL's as a function of their number above threshold.

SUMMARY

1. The measurement of an attribute of sensation in terms of a scale of numbers for which the usual laws of addition are valid can be achieved providing there exist operations for the determination of fractional values of the attribute.

2. Four methods are available for determining the half value in the case of the attribute loudness.

3. A loudness scale has been erected on the basis of the results of several experiments. It is proposed that the unit of the scale be the loudness of a 1000 cycle tone 40 db above threshold heard with both ears, and that it be called a *sone*.

4. In terms of this scale the subjective magnitude of the just noticeable difference has been measured. The size of DL's is different at different frequencies, and at a fixed frequency it is proportional to the 1.2 power of the number that the DL is above threshold.

[MS. received January 3, 1936]

THE SYSTEMATIC POSITION OF EMOTION

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The history of experimental and theoretical psychology clearly shows that the development of the science as a whole involved not only a gradual change in its outward contours, but also a continuous adaptation and readjustment of systemic parts within its confines. Slowly evolving major concepts like mind, association and volition, were accompanied by vital variations in the significance of such minor items as sensation, idea and feeling. Like the adolescent science that many of us are willing to admit psychology still is, the growth of the discipline has been manifestly somewhat uneven. Though tall in stature, psychology still moves awkwardly because its various parts are not only unequally developed, but are also poorly coördinated.

Most systematic psychologists will readily agree that for various reasons the affective and emotional aspects of experience have remained sadly under-developed. This situation has led not only to a dearth of facts and perhaps to an overemphasis of theory, but to a confusion of terminology and in some cases to an explicit or implicit 'throwing up of hands.' Max Meyer, for instance, says:

> The whale has a twofold distinction among the fishes: first, when seen from a distance, it looms large among them; and second, on close examination it is found to be no fish at all. Something like that I predict for the theory of emotions among the theories in psychological textbooks and periodicals.[1]

Bentley, in opening the notable symposium on feeling and emotion at Wittenberg College, doubted whether emotion was then more than the heading of a chapter in a textbook or a

[1] M. F. Meyer, That whale among the fishes—the theory of emotions, PSYCHOL. REV., 1933, 40, 292.

topic for discussion.[2] Dunlap wondered whether the emotions which psychologists were talking about were factual or mythological.[3] In compiling his dictionary, Warren had particular difficulty in finding technical meanings for terms commonly used psychologically in the field of emotion.[4] Such criticisms are not only fairly numerous, but any careful scrutiny of the theoretical and experimental literature easily reveals ambiguities in the meaning of fundamental concepts. The most frequent confusion comes from the uncritical use of the term 'emotion' when only *bodily* manifestations are referred to. From the point of view of psychological taxonomy, probably no finding is more significant in this connection than that of Cantril, Landis and Hunt when in two separate investigations they discovered that, both in normal and in abnormal subjects, the bodily symptoms resulting from the intramuscular injection of adrenalin chloride were not nearly as important as the cognitive awareness of some object or situation.[5] Thus 'cold' or 'quasi' and 'as if' emotions were again more definitely distinguished from real, full-fledged emotions on the basis of experienced factors which lay outside of the bodily symptoms or their effects than Marañon had succeeded in doing.

Obviously, then, more stress should be placed wherever it is possible on what might be called the bilateral approach to the subject of feeling and emotion. While the bodily expressions of various sorts are the most obvious and apparently the most 'objective,' many of the experimenters have increasingly become aware of the necessity of obtaining analytical accounts of the affective experience involved. Some of them are definitely convinced, as are the investigators above referred to, that the bodily components are secondary to the primary consideration of the cognitive phase of the

[2] M. Bentley, Is 'emotion' more than a chapter heading, Chap. 1, in 'Feelings and emotions, the Wittenberg symposium' (ed. by Reymert), 1928, pp. 21–23.

[3] K. Dunlap, Emotion as a dynamic background, Chap. 11, *ibid.*, pp. 150–160.

[4] H. C. Warren, Dictionary of psychology, Boston: Houghton Mifflin, 1934, pp. viii.

[5] H. Cantril and W. A. Hunt, Emotional effects produced by the injection of adrenalin, *Amer. J. Psychol.*, 44, 1932, 300–307; C. Landis and W. A. Hunt, Adrenalin and emotion, PSYCHOL. REV., 39, 1932, 467–485.

situation. Since we have long ago discovered that this cognitive aspect can not be safely inferred from the description of the stimulus or of the situation, as a physical event, and since the bodily components are merely 'accessory after the fact,' our only recourse is to the analytical reports of trained observers. This dilemma is strongly reminiscent of the famous reply to William James. Not just any bear would cause the reflex organic response but a bear that was *perceived* as dangerous or fearsome.[6]

From a systematic position, then, the present baseline of reference is composed, first, of all the conscious aspects of feeling, touched off usually by some cognitive process, though, as we shall see later, this cognitive process need not necessarily be an ingredient in the simpler forms of the affective life. An object, situation, event, or relationship is perceived or ideated and therewith comes the experience which we have learned to call feeling and emotion. Secondly there are wide-spread and often profound bodily 'reverberations,' involving many physiological systems.[7] This is not a chronological but an analytical and logical order. In an experimental situation both phases are subject to study and description.

It is with this picture in mind that a phylogenetic theory of feeling and emotion is proposed. Numerous writers have suggested that at the dawn of consciousness in single-celled animals the barest sort of feeling—mere comfortableness and uncomfortableness—might reasonably be posited.[8] No sensations as such could be present because no sensory apparatus is available. But according to our theory no special mechanism is required in this earliest phase of the affective life. The history of speculation on the subject reveals no agreement at higher levels of the biological scale as to specific neural or other structures which may be correlated with the affective

[6] W. L. Worcester, Observations on some points in James's Psychology, II. emotion, *Monist*, 1892, 3, 285–298.

[7] H. C. Warren and L. Carmichael, Elements of human psychology (rev. ed.), Boston: Houghton Mifflin, 1930, pp. 232–235.

[8] Among the more prominent psychologists who have advanced a similar hypothesis are Bain, Höffding, Horwicz, Lewes, Ribot, and Spencer. There are noticeable variations in their respective proposals but they all agree in presuming an affective beginning of consciousness.

experience. Titchener hypothecated the free nerve-endings [9] and Calkins [10] has referred to the frontal cerebral areas, while Troland,[11] Max Meyer,[12] Allport [13] and others have suggested special ways in which parts of the neural system function in connection with the affective process, as, for example, the interaction of the neurons or the antagonistic controls of the autonomic nervous chain. As compared with all the other simpler mental qualities that are required to explain the conscious behavior of primitive animals, it is not difficult to show that no special structure has ever been found which can readily be assigned to the feelings.

On the other hand, a considerable amount of converging evidence now exists for supposing that the simple feelings stand at the bottom of the ladder of conscious development. As part of the evidence we might indicate: (1) the early confusion of pain and unpleasantness, together with later and most recent attempts to translate the feelings into sensations either of organic or systemic types or of the 'pressury' variety with organic localizations, (2) the ambiguous systematic position of affection as process or attribute, (3) the inherent peculiar characteristics of affection in having only two distinct qualities with one of them named merely by means of the logical antonym of the other,[14] and its recognized uniqueness in being cognitatively or attentively inscrutable, 'subjective,' and indefinite. All told, such descriptions would well fit any process that defied comparison with later more highly developed processes. Indeed, under experimental settings, such presensory characteristics have recently been found by

[9] E. B. Titchener, The psychology of feeling and attention, New York: Macmillan, 1908, p. 292; A textbook of psychology, New York: Macmillan, 1919, p. 261.

[10] M. W. Calkins, A first book in psychology (4th rev. ed.), New York: Macmillan, 1914, pp. 211–214.

[11] L. T. Troland, The principles of psychophysiology, Vol. III, New York: Van Nostrand, 1932, pp. 233–282.

[12] M. Meyer, The nervous correlate of pleasantness and unpleasantness, Psychol. Rev., 1908, 15, 201–216; 292–322.

[13] F. H. Allport, A physiological-genetic theory of feeling and emotion, Psychol. Rev., 1922, 29, 132–139.

[14] C. A. Ruckmick, The psychology of pleasantness, Psychol. Rev., 1925, 32, 362–383.

Bichowsky.[15] They are similar to what Washburn has expected in some of the lower animal forms when she writes:

> This much evidence there is for saying that besides a possible food sensation, the Amœba may have some dim awareness of affective qualities corresponding to pleasantness and unpleasantness in ourselves. It should, however, be borne in mind that wide differences must go along with the correspondence. In us, pleasantness brings a thrill, a 'bodily resonance,' due to its tonic effect upon the circulation, breathing, and muscles; unpleasantness has also its accompaniment of vague organic sensation, without which we can hardly conceive what it would be like. In an Amœba, it is clear that this aspect, as found in human consciousness, must be wholly lacking. Again, in the human mind pleasantness and unpleasantness are connected with various sensation qualities or complexes; we are pleased or displeased usually 'at' something definite. The vagueness of the affective qualities in an Amœba's consciousness can only be remotely suggested by our own vague, diffused sense of bodily well-being or ill-being; and this is undoubtedly given its coloring in our case by the structure and functioning of our internal organs.[16]

There are other supporting reasons, especially from the field of child-psychology [17] and from the studies concerned with synæsthesia and eidetic imagery,[18] which justify the position that phylogenetically and ontogenetically affection may be considered as the most primitive and undifferentiated type of consciousness. Probably the most serviceable approach on the mental as well as on the physical side is the Coghill [19] hypothesis of gradual individuation and specificity of process. Let us say, then, that the simple feeling was the earliest representative of consciousness and coextensive with it—it and experience were equivalent. Slowly sensation

[15] F. R. Bichowsky, The mechanism of consciousness: pre-sensation, *Amer. J. Psychol.*, 1925, **36**, 588–596.

[16] M. F. Washburn, The animal mind (3rd ed.), New York: Macmillan, 1926, p. 41.

[17] W. Preyer, The mind of the child, Part I: the senses and the will (trans. by Brown), 2nd ed., New York: D. Appleton, 1884, p. 185; A. Gesell, Maturation and infant behavior pattern, PSYCHOL. REV., 1929, **36**, 307–319.

[18] H. Werner, Einführung in die Entwicklungs-psychologie, 1926, pp. 1–61.

[19] J. E. Coghill, The neuro-embryologic study of behavior: principles, perspective and aim, *Science*, 1933, **78**, 131–138; Anatomy and the problem of behavior, Cambridge, England: University Press, 1929.

emerged, at first indistinguishable from it, but taking on form as primitive sense-organs were provided. These sensations were not systematically different in reference: they were as subjective as the affective process, referring to the general condition of the body. The term 'common sensibility' or *Gemeingefühl* has been applied to this type of experience.

As specialized sense-organs arose and sensations began to be differentiated, an external reference to other objects became possible and likely. But an internal group remained, with its affective coloring as before. These new processes provided the next step in the development of the affective life: the appearance of the *sense-feeling*, of which hunger, thirst, fatigue, nausea, dizziness, and many aches are examples.

When sensations grew to be synthesized into perceptual forms, pertaining to objects, events, situations and various relationships, then for the first time emotions arrived on the scene as differentiated or differentiable mental experiences. Soon these became more complex with the introduction of memory and other forms of ideational activities, and there then followed the development of the sentiments, or what have been called the gentler or subtler emotions. Ideas, when they became available to the human mind, could set off emotional experiences on their own account, but, in their more abstract qualities or in their generalized forms, they were capable of developing the highest types of ethical, æsthetic and logical feelings.

In order to summarize the mental aspect of feeling and emotion and to pave the way toward a standardization of terminology, we may say that *feeling* or the *affective life* covers the whole gamut of this aspect of experience. *Simple feelings* or *affections* are the rudimentary experiences of a pre-sensory sort referable to the minds of the smallest single-celled animals as well as to the human mind and illustrated by comfortableness and uncomfortableness, agreeableness and disagreeableness, pleasantness and unpleasantness. Such experiences may have no other cognitive reference than the vague awareness of the condition of the organism itself. When

sensations are developed, as the next stage of individuation, affections may become attached to these processes and to all other higher mental processes in turn. But when they are more or less permanent aspects, invariably present with these sensory groups, then they may be called *sense-feelings*. The term *emotion* should be reserved for a rather complex experience which must include cognition on the perceptual level. Not single stimuli of the physical order, but patterns of stimuli, objects, events and situations must be cognized to produce an emotional affect. Later on ideas, coming into the perceptual situation gradually, are themselves capable of arousing emotions, but when they are the primary cognitive source, especially when the abstract qualities which they represent are emphasized, then we may speak of the *sentiments*. *Moods* are emotional variants of longer duration, weaker intensity, and unemphatic cognitive designation; *passions* are emotional variants of shorter duration, stronger intensity, and most emphatic cognitive designation. *Affect* refers to the bodily components of the emotional experience.

Now as to the other aspect of the bilateral description of the affective life: the bodily components. We have already touched upon this aspect briefly. The affective processes are notable for their wide-spread effects: the name emotion especially denotes this. The body is still to all intents and purposes a 'sounding board' or resonator for the affections. This function harks back to the primordial reference of bodily welfare. There is hardly a bodily structure which is not tied up with some affective response. From the very nature of feeling and emotion, movement of some kind has usually accompanied this sort of experience and has been extensively studied. Obviously movement as manifested by some form of motor apparatus is not the only desideratum but some form of neuromotor or neuromuscular system is usually required as a fundamental instrument. It was chiefly the supposed lack of such a basic mechanism in the lower organisms that made Höffding hesitate to accept feeling as a primordial phase of consciousness.[20] Since that time investigations have ade-

[20] H. Höffding, Outlines of psychology (trans. by Lowndes), London: Macmillan, 1891, pp. 96 *ff.*

quately disclosed a detailed neuromotor apparatus in the parasitic ciliate, *Diplodium ecaudatum*, in the fresh water ciliate, *Euplotes patella*, and in other protozoic forms.[21] Such a device helps to account for the periodic and automatic movements of the cilia, of the flagellum, and of other parts of the cell. It is probably the lowest type of nerve structure known. The next step in development is the nerve-net, or plexus, which has been identified in a great variety of animal forms from the cœlenterates to the vertebrates inclusively. It is found in the case of man in many internal organs like the heart, and the alimentary canal. The autonomy of the tentacles in sea-anemones, discovered by von Heider in 1879, furnished perhaps the first demonstration of the function of the nerve-nets in these lower forms of metazoa.[22] Without going into the matter, it is not difficult to trace the development of the entire present nervous system of man back to the simple nerve-nets which he still retains. The more detailed and highly specialized systems, the central and the autonomic, are again examples of forms of structural and functional individuation and specificity as compared with the more generalized forms found in the networks or even the earlier neuromotor apparatus.

The physiological picture of the increasingly specific form and function of the neuromuscular system again agrees almost point for point with the growing affective life. The pre-sensory type of feeling, having to do with the welfare of the body, could be correlated with the widely spread networks. Hampered or inadequately functioning networks would correspond with unpleasantness of that incognitive type. Smooth-functioning and efficient muscular systems as integrated by these nerve-nets would be related to pleasantness. This stage would be that of simple feeling, without the possibility of conscious bodily resonance on the sensory level. But when the body was itself supplied with sensory components, gradually the sense-feelings were provided for and

[21] R. G. Sharp, *Diplodium ecaudatum* with an account of its neuromotor apparatus, *Univ. Calif. Publ. Zoöl.*, 1914, 13, 43–122; C. V. Taylor, Demonstration of the function of the neuromotor apparatus in *Euplotes* by the method of microdissection, *Univ. Calif. Publ. Zoöl.*, 1920, 19, 403–470.

[22] G. H. Parker, The elementary nervous system, Philadelphia and London: Lippincott, 1919, p. 118.

the autonomic nervous system also came into play. When a more highly specialized sensorium developed and a correspondingly well-coördinated motorium came into being, the central nervous system was needed. Now sensory materials from outside objects and events could be affectively or algedonically colored. Such was the case very definitely with experiences of the perceptual order. Emotions *per se* could not have appeared in animals without a central nervous system developed to the point of including at least some type of cortex where higher coördinations are possible and where items from past experience could be incorporated.

We can not here go into the matter of the progressive development of the higher brain centers, but every consideration points to the cerebral areas for the final integration of cognitive processes into the emotional complex. There can be no question about that phase of the problem when we regard ideational material as also being operative in the more abstract and intellectualized emotions and sentiments.

We have a vast framework before us, then, in the assignment of stages of development to the neuromotor and neuromuscular systems. Parallel to that development we have the genetic growth of the affective life. Such a mode of correspondence with broad interpenetrating systems appears from every point of view more satisfactory than the allocation of emotion to the hypothalamus or any other single neural structure. Important though the hypothalamus may be, it can not be any more important than the neck of a bottle, or a switchboard in a telephonic system. Bodily effects of great variety and scope are after all involved, and a neuromotor, a neuromuscular, and a neuroglandular system are essential anatomical features to be investigated.

A re-statement of all these points condensed into a formula gives us something like the following:

(1) The affective life begins with consciousness itself in the lowest animal forms. The simple or elementary feelings or the affective processes are to be identified with this early form of experience. In other words consciousness is nothing more than feeling in the technical sense of the word.

(2) As the mental life develops, this elementary phase of consciousness spreads from whole to part in the sense that it becomes

attached to, that it permeates through, every succeeding phase of developing conscious processes. On the neural side, therefore, no special receptor or effector mechanism is required since none was needed in the first place when the animal was nothing more than a single cell. It uses, however, all existing channels for the conveyance of electrochemical energy, especially those systems like the neuromotor apparatus, the nerve-nets or plexuses, the autonomic networks and the central nervous system, which most speedily transmit energy from the whole organism to the part.

This theory, which involves both mental and bodily aspects and which, therefore, meets our specification of a bilateral approach, does not pretend to be anything more than a current hypothesis. It does what any theory is supposed to do, namely, it advances a systematic generalization based upon research to date. If our generalization is wrong, subsequent investigations will prove it so. Such a theory not only provides a synthesis of all that has gone before by way of theoretical and experimental study, but it furnishes us with a basis of departure for further experimentation. For a long time the area of feeling and emotion has lacked an organic systematization either by way of terminology or by way of physiological explanation. Terminology may be standardized by specific proposal, but this standardization can then only become tentatively fixed through continued usage.

The neuromotor explanation does not involve any notion of specific structure or functional allocation since this lack of definite localization is a characterization of the whole feeling process, which was originally vague and all-pervasive and remains so as part of its general nature. Details concerning qualitative distinctions, such as pleasantness and unpleasantness, must still be worked out further. For the most part we should probably subsume a biological explanation in terms of a smooth-running, forward-moving and uninhibited organism, or any part thereof, for the pleasant quality, and, on the other hand, an obstructed, blocked and temporarily disarranged organism, or any part thereof, for the opposite quality of unpleasantness. This may easily be done in terms of some neuromotor theory such as we have advanced.

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THE NATURE OF DISCRIMINATION LEARNING IN ANIMALS [1]

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I

Until recently the learning of a discrimination problem by an animal usually was regarded as an unavoidable nuisance preliminary to the study and determination of sensory capacities, and the experimenter's interest in it was more or less limited to devising means of speeding it up in order to proceed as quickly as possible to the main task of determining sensory thresholds. Largely as the result of a suggestion by Lashley (11), however, there has been a definite shift of interest and the discrimination method is now being employed to considerable advantage in studying the learning process itself, particularly the characteristics of the pre-solution period of learning.

Lashley's suggestion [2] was that the behavioral phenomena commonly observed in the setting up of discrimination habits, such as persistent responses to position or other irrelevant stimuli, might represent 'attempted solutions' of the problem by the animal and that if this were the case then the trials preceding solution were in all probability 'irrelevant to the actual formation of the association' (11). Acting upon this suggestion Krechevsky carried out a series of investigations

[1] The writer is greatly indebted to Professors Clark L. Hull and Henry W. Nissen for many helpful criticisms and suggestions.

[2] Hamilton (1) and Yerkes (19) had previously called attention to the systematic character of the responses of animals in the solution of multiple and quadruple choice problems. MacGillivary and Stone were the first to make an analysis of the systematic modes of response made by the white rat in the acquisition of the discrimination habit. Their study showed the importance of both a perseveration and an alternating tendency and led them to conclude "that 'trial-and-error learning,' so-called, usually does not depict the operation of chance factors alone but rather displays the operation of one or more directive tendencies other than the one that will eventually lead to mastery of the problem." (13, 489.)

of discrimination learning with the white rat (6, 7, 8, 9, 10), in which he showed, by an appropriate analysis of individual records, that the reactions of the animal during the pre-solution period were not a chance affair but consisted of systematic modes of response—position, alternation, etc., until the correct solution was discovered. He termed these systematic pre-solution responses 'hypotheses' and, like Lashley, interpreted them as attempts at solution of the problem by the rat, with the implication that they represented a kind of behavior superior in some manner to that usually described as 'trial-and-error.' Furthermore, he concluded that these experimental facts offer conclusive evidence of the inadequacy of 'trial-and-error' theories of learning, because, as he claimed, they contradict the assumption made by the latter that the learning process is, in its early stages, random and haphazard in nature.

While such facts as Krechevsky presents undoubtedly do not fit the description of the early part of learning as consisting of purely random responses, they do not necessarily refute 'trial-and-error' theories of learning for the simple reason that the assumption as to the chance character of the responses in learning is not necessary to this type of theory. The assumption has existed, and for some reason continues to exist, only in the minds of its opponents, for even the earliest crude versions of Thorndike (16) and Watson (18) did not hold that the responses made to a problem situation were really random, but pointed out that they were limited to those that the animal, in its previous experience, had learned were relevant to the situation. Similarly, a more recent formulation of this type of theory of learning very definitely states that "The range or variety of reactions which may be evoked by a given problem situation is limited to the reactions which have become conditioned during the life of the organism to one or another stimulus component of that situation" (2), plus, of course, any native or inherited responses.

In a like manner, a sophisticated 'trial-and-error' theory of learning would not hold that the order of sequence of trial acts is a haphazard affair, but rather would conceive it as

proceeding according to definite principles or laws. Recent 'trial-and-error' learning theories recognize full well that it is an organized, lawful, systematic process. As a matter of fact, the theoretical concepts they employ are put forward on this very assumption. Nevertheless, it must be admitted that as yet no learning theory of this type has presented an adequate account of the phenomena of discrimination learning. For the most part they have merely outlined in very general terms the mechanisms underlying this behavior. It is the purpose of the present paper to attempt a theoretical account of discrimination learning founded on 'trial-and-error' principles similar in nature to those recently exploited by Hull in the field of problem-box and maze learning (**2**, **3**, **4**).

II

In one of the earliest of his recent series of theoretical articles Hull (**2**) pointed out that there are several fairly distinct kinds of 'trial-and-error' learning. Thus he differentiated the complex type exemplified by maze learning, in which the reinforcement or reward comes only at the end of a series of trial acts, from a simple type in which each trial act is reinforced immediately if successful, but is followed either by no special stimulus or by punishment if unsuccessful. Problem-box learning is an example of the latter, simple type, since there are several alternative trial acts possible to a non-varying situation, only one of which is correct and thus followed by reinforcement.

Discrimination learning is also of this latter type, *i.e.*, each act is reinforced immediately if correct, and not if incorrect. However, discrimination learning is quite different from problem-box learning as further comparison will reveal. Thus, in problem-box learning only one of the alternative responses is ever followed by reward, the others never being reinforced; whereas in discrimination learning the same response may one time be rewarded and, at another time, not be rewarded, depending upon whether the stimulus aspect that determined it does or does not happen to coincide with the correct or cue aspect of the situation. Discrimination

learning does not consist, then, in the strengthening of one response relatively to another or others as is the case in problem-box learning, but involves, rather, the relative strengthening of the excitatory tendency of a certain component of the stimulus complex as compared with that of certain other elements until it attains sufficient strength to determine the response. By the conditions of the experiment the relevant stimulus component is always reinforced and never frustrated, whereas, irrelevant components receive both reinforcement and frustration.

Before proceeding to the analysis of this process, however, a discussion will be presented of the main theoretical principles that are to be employed. The first of these is the principle of reinforcement. This principle, as it will be used here, assumes that if a reaction is followed by reward, which may be defined in terms of the occurrence of a final or consummatory response, the excitatory tendencies of the immediate stimulus components are reinforced or strengthened by a certain increment, '*I*.' It will be noticed that the statement of this principle is a general one involving no assumptions as to the nature of the reinforcing mechanism. The second principle to be used is that of inhibition or frustration, which states that when a reaction is not rewarded, *i.e.*, when the final or consummatory response is prevented from taking place, the excitatory tendencies of the active stimulus components are weakened by a certain decrement, '*D*.' It assumes that this weakening is due to an active, negative process, inhibition, which, adding itself in algebraic fashion to the positive excitatory tendencies, results in lowered strength values.

Certain further assumptions have also been made as to the relative amounts of strengthening and weakening the excitatory tendencies or $S \cdots R$ connections undergo with reinforcement and non-reinforcement, particularly as to variations in amount in different stages of the learning process. Because of the lack of adequate experimental data bearing directly upon this problem it has been necessary to choose more or less arbitrarily from among the numerous possibilities.

Wherever possible, however, an attempt has been made to select those assumptions that best fit the available experimental evidence. For example, in considering the amount of strengthening resulting from reinforcement, the studies of the shape of the curve of acquisition of the conditioned response have been taken into consideration. Hull (**5**), in reviewing the evidence from these studies, concluded that this curve is probably S-shaped, which suggests that the relative strengthening effect of a single reinforcement is least at the beginning and end of the learning and greatest in the middle portion. If, then, one were to plot the amount of strengthening resulting for each reinforcement from the beginning of the learning process necessary to produce such an S-shaped curve it would look like the common bell-shaped distribution curve used by statisticians. The S-shaped learning curve is simply the integral of such a normal probability curve, representing the cumulative effects of successive reinforcements. Accordingly, the assumption has been made that the strengthening effect of reward varies with the stage of acquisition or strength of the excitatory tendency in this manner. Assuming that the strength of any $S \cdots R$ connection (or excitatory tendency of an S to arouse an R) varies between the limiting values 0 and 100, the strengthening effect of a single reinforcement at various stages of acquisition is assumed to be as in column 2 of Table I, the values of which are determined from the function I (increment of strength) $= \frac{3.99}{e^{(s-50)^2/555}}$ in which the values 3.99 and 555 are arbitrary constants dependent upon the individual learner and 's' is a variable representing the strength of the $S \cdots R$ connection at any point in the learning process. Thus when an $S \cdots R$ connection has a strength (s) of 20, one reinforcement increases its strength 0.79 giving it a value of 20.79.

Concerning the weakening effect of an $S \cdots R$ connection of failure of reward the little evidence that exists suggests that it varies directly with the strength of the response, being greater for strong ones than for weak ones.[3] Accordingly it

[3] It should be particularly noted here that we are referring only to the failure of reward, *i.e.*, failure of the consummatory response to take place, and not to punishment

TABLE I

SHOWING THE STRENGTHENING AND WEAKENING EFFECTS OF REINFORCEMENT AND NON-REINFORCEMENT FOR VARIOUS STRENGTHS OF EXCITATORY TENDENCIES OR S···R CONNECTIONS

Strength of Excitatory Tendency (s)	Strengthening Effect of a Single Reinforcement $I = \frac{3.99}{555}(s-50)^2$	Weakening Effect of a Single Failure of Reward $D = .05\,s - .5$
1	.05	.00*
5	.10	.00*
10	.22	.00
15	.44	.25
20	.79	.50
25	1.30	.75
30	1.94	1.00
35	2.66	1.25
40	3.33	1.50
45	3.81	1.75
50	3.99	2.00
55	3.81	2.25
60	3.33	2.50
65	2.66	2.75
70	1.94	3.00
75	1.30	3.25
80	.79	3.50
85	.44	3.75
90	.22	4.00
95	.10	4.25
99	.05	4.45

* 'D' is assumed to be zero for all values of 's' less than 10.

has been assumed that the strength of an $S\cdots R$ connection is decreased or weakened by failure of reward a decrement, $D = .05\,s - .5$, in which the values .05 and .5 are constants for an individual learner and 's' is the strength of the $S\cdots R$ connection. These values are given in the third column of Table I. The selection of a straight line function is for the sake of simplicity. It is quite possible that the function is some type of curvilinear one, *e.g.*, logarithmic.

The selection of the values for the constants has, of course, been purely arbitrary. It will be observed that the relative

such as the administration of electric shock. The latter, according to the present view, involves an entirely different mechanism. That is, shock would not be conceived as weakening an S···R connection it followed, but as strengthening an opposing one. Thus if a particular stimulus through training had acquired an excitatory tendency eliciting an approaching response, the introduction of electric shock following its occurrence would not weaken this excitatory tendency but would build up or strengthen the excitatory tendency to the opposing response of withdrawal.

effects of these two processes have been arbitrarily weighted by the values given to the constants in the two functions so that the maximum amount of reinforcement, that at the point 50, is not greatly different (3.99 and 4.50) from the maximum amount of inhibition, which occurs at the point 100. It is quite possible, of course, that in some individuals the inhibition will be relatively much weaker than the reinforcing function, while in others the opposite will be the case, inhibition being relatively much stronger. The present case is selected merely for purposes of exposition.

A summary of these two theoretical principles including the assumptions as to the nature of the variation of '*I*' and '*D*' with the stage of the learning process are presented below:

(1) The strength of an $S \cdots R$ connection is increased when followed by reward, *i.e.*, the occurrence of a final or consummatory response, by an increment, '*I*,' which varies according to the function $I = \frac{K}{e^{(s-50)^2/2t^2}}$, in which K and t are constants dependent upon the individual learner, and s is a variable representing the strength of the $S \cdots R$ connection at any point in the learning process.

(2) The strength of an $S \cdots R$ connection is decreased or weakened by failure of reward by a decrement, '*D*,' which varies according to the function $D = as - b$, in which a and b are constants for an individual learner and s is a variable representing the strength of the $S \cdots R$ connection at any point in the learning process.

In addition to these two general principles the following basic assumptions or postulates will also be made:

(3) The strength of an $S \cdots R$ connection or of the excitatory tendency of an S to arouse an R varies between the limiting values 0 and 100.

(4) In the case of antagonistic, *i.e.*, mutually exclusive, stimulus-response connections there will result a competition in which that having the greatest strength will prevail.

(5) The total excitatory strength of a stimulus complex is the sum of the excitatory tendencies of the component stimuli.

III

With these theoretical principles or postulates outlined we may now turn our attention to the analysis of the discrimination problem. The various stimulus aspects in the discrimination situation are, undoubtedly, considerable in number. The essential ones in producing the selective response, however, are the visual appearances of the two boxes or doors containing the incentive, and the two stimuli to be differentiated, *e.g.*, circular and triangular forms. It may be assumed that the organism has acquired, in its past experience, reaction tendencies of orientating towards and approaching each of these stimuli so that we have the following four $S \cdots R$ connections: S_{LB} (left box)$\cdots R_A$ (approaching response), S_{RB} (right box)$\cdots R_A$ (approaching response), S_c (circular form) $\cdots R_A$ (approaching response), and S_t (triangular form)$\cdots R_A$ (approaching response). That is to say, it may be assumed that at the beginning of the discrimination experiment the organism already has these $S \cdots R$ connections established in some degree and that, depending upon the amount and kinds of previous experience, they will each have certain, finite strengths between 0 and 100.[4] The situation may be represented schematically as follows, assuming the circular or position component of the stimulus complex to be situated on the left box and the triangle on the right.

$S_{LB} \ldots\ldots R_A$
$S_c \ldots\ldots R_A$
$S_t \ldots\ldots R_A$
$S_{RB} \ldots\ldots R_A$

Examination of this stimulus response scheme will show that the four stimulus components are so arranged or paired

[4] It will perhaps be claimed that when one begins with the assumption that the animal has already acquired this response tendency to each of these stimulus aspects no real explanation of the learning process has been provided because one has already assumed what was to be explained. It should be noted, however, that our present purpose is to account for the behavioral phenomena characteristic of the learning period of the discrimination habit, *i.e.*, the acquisition of the differential character of the animal's behavior so that it always approaches a certain component of the stimulus complex, and not to give an account of the development of the animal's stimulus-reaction system prior to this period. Observation of the behavior capacities of animals at the beginning of experimental training should make it obvious, moreover, that the assumption that they do have such stimulus-response tendencies established in some degree at least, is not a purely gratuitous one.

that there are only two opposed or competing sets of excitatory tendencies; in this instance, those of the circular form and left box and those of the triangular form and right box. Now which of these opposing sets of stimulus components the animal will respond to on any particular trial by approaching, will depend upon which has the greater aggregate of excitatory tendencies eliciting such a response. Thus if we assume in the case of a hypothetical animal just beginning to learn the problem, that the excitatory strengths of the four stimulus components to the approaching response are as follows:[5] S_{LB} = 80.20, S_{RB} = 80.00, S_c = 10.00, and S_t = 10.00, then the combined excitatory tendency of S_{LB} and S_c is 90.20 (*i.e.*, 80.20 + 10.00) while the combined strength of S_{RB} and S_t is 90.00 (*i.e.*, 80.00 + 10.00). The combination of the former two stimuli, S_{LB} and S_c, being the stronger, the animal would, other things being equal, respond by approaching them and, being correct, would receive reward with the result that the excitatory tendencies of these two stimulus components to elicit the approaching response would be strengthened, S_{LB} to 80.97 and S_c to 10.22, according to calculations based on Table I. If, on the following trial, the positive cue stimulus (S_c) is on the right box, the aggregate excitatory tendency of these stimuli to the approaching response would be 90.22 (*i.e.*, 80.00 + 10.22), while that of S_t and S_{LB} would be 90.97 (*i.e.*, 80.97 + 10.00) with the result that the animal would go to the latter stimuli. On this occasion, however, the response would be incorrect and with non-reward there would develop inhibition or a net weakening of the excitatory tendencies of

[5] The particular values assumed here are more or less arbitrary. They were chosen as probably characteristic of a sophisticated subject, *i.e.*, one that is familiar with the experimental situation as the result of learning a previous discrimination problem. Thus the strengths of the box stimulus components are very high as compared with cue stimuli. Presumably, unsophisticated subjects also begin with the box or position stimuli somewhat stronger than the cue stimuli. At least the large majority of animals show a definite tendency for spatial habits or responses to position to be more dominant than habits based on other sensory characteristics. Possibly this is a function of native or inherited factors, although the cage life of laboratory animals might provide for this difference. Certainly most animals have little opportunity to develop responses to such visual characteristics as triangles, circles, squares, etc. On the other hand they might show fairly strong and differentially developed responses to light and dark stimuli. The past experience of the animal is an extremely important factor in this connection and must always be taken into full account.

these stimulus components.[6] The excitatory tendency of S_{LB} to R_A would be weakened to 77.42, while that of S_t would remain 10.00 as inhibition, according to our assumptions (Table I), does not develop at this strength.

In Table II is presented a complete account of the learning process in this hypothetical case. The first two columns of this table show the number of the trial and the position of the positive stimulus. The third column gives the combined strengths of the excitatory tendencies leading to the response of approaching the stimuli at the left box, which on one trial may be S_e and S_{LB} and on another S_t and S_{LB}, as shown in the following three columns. The same data for the response of approaching the stimuli at the right box are given in the next four columns. The final columns indicate to which set of stimuli the approaching response is made and whether this response is correct or not, an incorrect choice being bracketed. The consequences of the responses upon the strengths of the various excitatory tendencies always appear in the next row beneath, which represents their status for this ensuing trial. With the successive reinforcements of the excitatory tendencies of S_e and the failure of reinforcement of those of S_t, the difference between their strengths becomes gradually larger until sufficiently great to offset, consistently, any other differences that exist between the two competing sets of stimulus components. Learning is completed only when this stage is reached. Thus at the seventeenth trial it will be seen that although the difference between S_e and S_t is 2.04 points, this

[6] The attention of the reader is here directed to a very important point, namely, that in our hypothetical case the experimental conditions are arranged such that there is no reinforcement whatever following an error. The incorrect box is either locked or without food, and most important of all, the training procedure does not permit the subject to go immediately to the alternative box after the incorrect one is chosen. Instead, the animal is taken out of the situation and, after an interval, given another trial. There is considerable reason to doubt whether, under the procedure of permitting the animal to respond immediately to the correct box after failure, there would develop an inhibition of the excitatory tendencies to the incorrect response. That is, almost immediate reinforcement of the correct response might be expected to disperse this inhibition; and it is conceivable that it might even result in an actual strengthening of these excitatory tendencies, although to a lesser degree than those leading to the correct response, which is always closer in time to the reinforcement (goal-gradient).

TABLE II

SHOWING A HYPOTHETICAL CASE OF LEARNING IN WHICH THE INITIAL EXCITATORY TENDENCIES ARE ASSUMED TO BE AS FOLLOWS: $S_{LB} = 80.20$, $S_{RB} = 80.00$, $S_e = 10.00$, AND $S_i = 10.00$

Trial	Pos. (+) S	Left	Strength of Component Excitatory Tendencies						Right	Response
			R_A			R_A				
			S_e	S_i	S_{LB}	S_e	S_i	S_{RB}		
1	L	90.20	10.00		80.20		10.00	80.00	90.00	L
2	R	90.97		10.00	80.97	10.22		80.00	90.22	(L)
3	R	87.42		10.00	77.42	10.22		80.00	90.22	R
4	L	87.87	10.45		77.42		10.00	80.80	90.80	(R)
5	L	87.87	10.45		77.42		10.00	77.25	87.25	L
6	R	88.45		10.00	78.45	10.67		77.25	87.92	(L)
7	L	85.69	10.67		75.02		10.00	77.25	87.25	(R)
8	R	85.02		10.00	75.02	10.67		73.90	84.57	(L)
9	R	81.77		10.00	71.77	10.67		73.90	84.57	R
10	L	82.71	10.94		71.77		10.00	75.35	85.35	(R)
										4
11	L	82.71	10.94		71.77		10.00	72.10	82.10	L
12	R	83.50		10.00	73.50	11.20		72.10	83.30	(L)
13	R	80.37		10.00	70.37	11.20		72.10	83.30	R
14	L	81.84	11.47		70.37		10.00	73.75	83.75	(R)
15	L	81.84	11.47		70.37		10.00	70.60	80.60	L
16	L	84.00	11.75		72.25		10.00	70.60	80.60	L
17	R	83.90		10.00	73.90	12.04		70.60	82.64	(L)
18	R	80.70		10.00	70.70	12.04		70.60	82.64	R
19	L	83.04	12.34		70.70		10.00	72.45	82.45	L
20	R	82.55		10.00	72.55	12.65		72.45	85.10	R
										7
21	R	82.55		10.00	72.55	12.99		74.05	87.04	R
22	L	85.88	13.33		72.55		10.00	75.47	85.47	L
23	L	87.84	13.69		74.15		10.00	75.47	85.47	L
24	R	85.55		10.00	75.55	14.06		75.47	89.53	R
25	R	85.55		10.00	75.55	14.45		76.72	91.17	R
26	L	90.41	14.86		75.55		10.00	77.82	87.82	L
27	R	86.80		10.00	76.80	15.28		77.82	93.10	R
28	L	92.54	15.74		76.80		10.00	78.82	88.82	L
29	L	94.12	16.22		77.90		10.00	78.82	88.82	L
30	R	88.90		10.00	78.90	16.73		78.82	95.55	R
										10

differential is not sufficient to offset the difference of 3.30 between S_{LB} and S_{RB}. The combined excitatory tendency of the correct stimulus components, *i.e.*, those of the right box, of 82.64, is not as great as that of the incorrect ones, *i.e.*, those at the left box, of 83.90, with the result that the response is made to the latter.

The above account does not, of course, tell the whole story. In order to simplify the exposition such factors as

disuse and spontaneous recovery from inhibition have been purposely neglected. These factors undoubtedly operate to complicate the learning process and they must ultimately be taken into consideration. Similarly, there are various other factors, functions of the experimental situation, that usually have to be considered. For example, learning may be delayed markedly if the connection between the relevant stimulus and the required motor response is not sufficiently obtrusive and clear to the animal. The mere presence of the cue stimulus somewhere in the experimental situation does not guarantee its impingement on the animal's sensorium at or near the critical moment of response. This was, undoubtedly, an important, if not the chief reason for the greater difficulty that was experienced in setting up discrimination habits in the older forms of indirect apparatus. The more recent types, such as those of Lashley (12) and Munn (15), are arranged in such a manner as to insure to a very high degree that the cue stimulus operates each trial.

IV

In the preceding section a hypothetical picture or logically possible account of the nature of discrimination learning has been outlined in terms of 'trial-and-error' principles of reinforcement and inhibition. An attempt will now be made to show how, on the basis of these assumptions, it is possible to explain (deduce) the various behavioral phenomena characteristic of this kind of learning. This section will be particularly concerned with such behavioral facts as Krechevsky and others have found to characterize the pre-solution period of discrimination learning and also the behavior of animals in an insoluble discrimination problem.

The explanation or deduction of the appearance of systematic responses at the very outset of the learning, such, for example, as the spatial habit of always choosing the left box, is fairly simple and obvious. If the excitatory tendency of one of the position stimulus components, *i.e.*, one of the food boxes, S_{LB} or S_{RB}, is, either because of innate factors or past training, much stronger than the other, the response will

consistently be made to the stronger, except as chance factors operate to override the difference. This systematic position habit will persist until the difference between excitatory tendencies of these stimulus components becomes, through the action of reinforcement and non-reinforcement, more or less eliminated, or until the difference between the strengths of the excitatory tendencies of the positive and negative cue components becomes sufficiently great to offset it. In the latter case, it should be noticed, the animals would be expected to fall back to a position response if the cue stimuli are removed, a phenomenon well known to everyone who has carried out such experiments.

Position responses, to the right or left, represent by far the most common systematic responses made by animals when first introduced to the discrimination problem. If, however, the excitatory tendencies of the competing stimulus components are not sufficiently different in strength to result in such a preferential tendency one would expect that a 'perseverance' habit would be adopted, the animal tending to repeat the response last correct and to continue choosing it until it is incorrect, when it would shift to the alternative because of the shift in balance of strength of the excitatory tendencies. In a similar manner, other systematic responses at the beginning of learning are explained in terms of innate or acquired differences in the excitatory strengths of one of the position or spatial stimulus components.

More difficult to account for than the presence of a systematic position response at the very beginning of learning is the development of such a habit after an initial period of no apparent preference or of one of an opposite tendency. The occurrence of the positive stimulus on the same side several times in succession, say two or three, might be expected to give a temporary advantage to this particular spatial component of the stimulus complex. One would expect it to be merely temporary, for with the subsequent greater number of trials on the opposite side this difference in their strengths should again be equalized. However, it should be noticed that whether such equalization will be attained depends upon

the stage of acquisition of the $S_{RB} \cdots R_A$ and $S_{LB} \cdots R_A$ connections. If these latter are relatively weak, as presumably would be the case with unsophisticated subjects, there would be or soon would develop, a considerable discrepancy between the strengthening and weakening effects of the reinforcing and inhibitory mechanisms, the former becoming much greater in value than the latter. Such a discrepancy would soon lead to the setting up of a preferential position habit, for with the presentation of the positive cue stimulus in the same position two or three times in succession the excitatory tendencies of this spatial component of the stimulus would be increased in strength to such an extent over the other that the greater number of subsequent non-reinforcements of responses to it would not reduce its strength sufficiently to equate them once more. In contrast, with sophisticated experimental subjects, the excitatory strengths of the position stimulus components (S_{LB} and S_{RB}) are relatively high as the result of past experience, and the subject would not tend to develop such strong, persistent position habits, as the inhibitory mechanism is, at this stage, comparable in strength to the reinforcing mechanism, being even stronger beyond a certain point. The balance between these two mechanisms would tend to maintain the equalization of the excitatory tendencies leading to the two boxes and thus prevent the establishment of any great difference between them.

Tables II and III show these two contrasting cases, the latter representing portions of the hypothetical learning of an unsophisticated subject while the former shows the learning of the experienced subject. An examination and comparison of these two tables reveals a considerable number of interesting phenomena. First of all, it will be observed that a persistent position habit does not develop in the case of the sophisticated subject (Table II), but is broken up immediately with failure on any temporarily preferred side, the response becoming one of perseveration with success and alternation upon failure until the solution of the problem. The unsophisticated subject, however, soon develops a considerable difference in the excitatory strengths of the two position stimulus com-

TABLE III

SHOWING A HYPOTHETICAL CASE OF LEARNING IN WHICH THE INITIAL EXCITATORY TENDENCIES ARE ASSUMED TO BE AS FOLLOWS: $S_{LB} = 35.00$, $S_{RB} = 35.20$, $S_e = 8.00$, $S_i = 7.00$

Trial	Pos. (+) S	Left	Strength of Component Excitatory Tendencies						Right	Response	
			R_A			R_A					
			S_e	S_i	S_{LB}	S_e	S_i	S_{RB}			
1	L	43.00	8.00		35.00		7.00	35.20	42.20	L	
2	R	44.66		7.00	37.66	8.17		35.20	43.39	(L)	
3	R	43.28		7.00	36.28	8.17		35.20	43.39	R	
4	L	44.62	8.34		36.28		7.00	37.88	44.88	(R)	
5	L	44.62	8.34		36.28		7.00	36.48	43.48	L	
6	R	46.12		7.00	39.12	8.52		36.48	45.00	(L)	
7	R	44.67		7.00	37.67	8.52		36.48	45.00	R	
8	L	46.37	8.70		37.67		7.00	39.35	46.35	L	
9	R	47.70		7.00	40.70	8.70		39.35	48.05	R	
10	L	49.58	8.88		40.70		7.00	42.61	49.61	(R)	
											6
11	R	47.70		7.00	40.70	8.88		40.98	49.86	R	
12	L	49.77	9.07		40.70		7.00	44.43	51.43	(R)	
13	L	49.77	9.07		40.70		7.00	42.70	49.70	L	
14	R	51.12		7.00	44.12	9.26		42.70	51.96	R	
15	R	51.12		7.00	44.12	9.45		46.32	55.77	R	
16	L	53.79	9.65		44.12		7.00	50.20	57.20	(R)	
17	L	53.77	9.65		44.12		7.00	48.20	55.20	(R)	
18	R	51.12		7.00	44.12	9.65		46.30	55.95	R	
19	L	53.98	9.86		44.12		7.00	50.18	57.18	(R)	
20	R	51.12		7.00	44.12	9.86		48.18	58.04	R	
Trials 21–80 every response to right with consequent 50 per cent success.											6
81	L	69.01	24.89		44.12		7.00	65.41	72.41	(R)	
82	R	51.12		7.00	44.12	24.89		62.64	87.53	R	
83	R	51.12		7.00	44.12	26.17		65.64	91.81	R	
84	L	71.73	27.61		44.12		7.00	68.22	75.22	(R)	
85	L	71.73	27.61		44.12		7.00	65.31	72.31	(R)	
86	R	51.12		7.00	44.12	27.61		62.55	90.16	R	
87	R	51.12		7.00	44.12	29.23		65.56	94.79	R	
88	L	75.21	31.09		44.12		7.00	68.15	75.15	L	
89	R	54.85		7.00	47.85	33.18		68.15	101.33	R	
90	L	83.43	35.58		47.85		7.00	70.36	77.36	L	
											7
91	R	58.80		7.00	51.80	38.12		70.36	108.48	R	
92	L	93.01	41.21		51.80		7.00	72.25	79.25	L	
93	L	100.44	44.68		55.76		7.00	72.25	79.25	L	
94	R	66.51		7.00	59.51	48.46		72.25	120.71	R	
95	R	66.51		7.00	59.51	52.43		73.89	126.32	R	
96	L	115.89	56.38		59.51		7.00	75.33	82.33	L	
97	L	122.99	60.08		62.91		7.00	75.33	82.33	L	
98	R	72.86		7.00	65.86	63.40		75.33	138.73	R	
99	L	132.16	66.30		65.86		7.00	76.59	83.59	L	
100	R	75.39		7.00	68.39	68.78		76.59	145.37	R	
											10

ponents and consequently adopts a position habit. It is extremely interesting to note, moreover, that as training continues this difference increases for the reason that the reinforcements have the greater effect, although they equal the frustrations in number of occurrences. It is only when the difference between the excitatory strengths of the positive and negative cue stimuli has reached an amount greater than the difference between the strengths of the position stimuli that the animal breaks over to the non-preferred side. This represents, of course, solution of the problem.

Assuming that such unconsidered factors as forgetting, spontaneous recovery from inhibition, and various chance factors will not change this theoretical picture to any significant extent, we have here an interesting and easily tested deduction, namely, that an animal that develops and maintains a long persistent position habit will, when it finally abandons it, proceed directly to the correct discrimination response. Or, in the terminology of Krechevesky, animals that persist for a considerable time in a position hypothesis will, upon rejecting it, immediately adopt the correct hypothesis without trying others, such as perseveration with success and alternation upon failure.

In his series of experiments on discrimination learning in rats, Krechevsky introduced and made considerable use of an insoluble problem, in which no one stimulus, color, form or position, was regularly the correct one. The development of a systematic mode of response in such a situation was interpreted by him as evidence that the animal, in part at least, must initiate or originate responses and that they are not simply forced upon it 'ab extra.' While one must grant that the presence or development of such systematic responses demands that something within the organism itself is differentiated or becomes so, for the experimental situation at no time contains a selective factor, nevertheless, it is not at all necessary to ascribe to the animal the powers that Krechevsky's interpretation suggests. It can be easily demonstrated that the theoretical structure developed above would lead to the expectation that an animal would adopt systematic modes

of response in such an unsolvable situation. Thus the large majority of these hypotheses are positional, to the right or to the left, which, because of their very quick development suggests that the excitatory strengths of one of the position stimuli, *i.e.*, S_{RB} or S_{LB}, is stronger than the other at the beginning of the experiment. The presence of a dark-going habit at the very beginning of the experiment would similarly be explained as a difference in strength between the dark and light stimulus excitatory tendencies. But even without the advantage of such assumed, initial differences between the strengths of the various stimuli it can be shown that such systematic modes would still be expected. The demonstration is similar to that in the case of the development of position habits after an initial period of no apparent preference in the regular discrimination problem.

V

In interpreting the results of his studies on discrimination learning, Krechevsky described the behavior of the rat during the pre-solution period as consisting of a series of 'hypotheses.' This name was chosen, he explains, because it suggested four behavioral characteristics describable by the terms (1) systematic, (2) docile, (3) selective, and (4) self-initiated. Now obviously such terms merely provide a description and name for the phenomena. They do not greatly further our understanding of them. That Krechevsky, himself, recognizes this is indicated by the following quotations from a joint publication by himself and Tolman (17), in which they discuss the relationship between their concepts of 'means-end-readiness' and 'hypothesis.'

> Thus it is to be strongly impressed upon the reader that all such terms as those of 'desire,' 'aversion,' 'expectation,' 'hypothesis,' 'means-end-readiness' are merely names, or concepts, for relationships between variables. They do not say anything about the *vera causa* of these relations. (17, p. 63.)

Finally, in closing, it may be pointed out that this descriptive problem of the behavior of means-end-readiness and hypothesis in learning and unlearning is but one phase of the total problem as to

all causal conditions underlying the determination and appearance of means-end-readinesses and hypotheses. What, for instance, brings about the initial appearance of certain subordinate hypotheses rather than others, and what are the causes of the alternations between one subordinate hypothesis and another. (17, pp. 68–69.)

One could certainly have no quarrel with this point of view, except, perhaps to protest the use and redefinition of terms which already have connotations that go beyond the limitations they set upon them, for it is an unfortunate fact that most readers quickly forget the new definition of the terms and very soon are interpreting them in all of their old meanings. In fact, Krechevsky, himself, seems, at times, to have departed from the strict definition of his description and has tended to think of the hypotheses as being purposive and insightfully 'attempted solutions' in the more generally accepted sense of such terms. This is most apparent in the attempts he has made to indicate the difference between his view and the old 'trial-and-error' interpretations. The latter, he insists, err in viewing the behavior as consisting of blind, non-docile habits to the external situation. According to him the responses of the animal are to be conceived of as something beyond this, in some way superior to it, having the character of insight.

But whatever Krechevsky's view of the matter may be, one can, nevertheless, advance the hypothesis that the animal is an organism, capable of insight, that sets out to solve the problem confronting it by trying out, systematically, one of its repertoire of hypotheses, which, failing of solution, leads it to initiate and try out another and another until the correct one is hit upon. Although such a view ignores such critical questions as to what constitutes failure of a hypothesis, and how this leads to its abandonment and the trying out of another, and what determines the order of attempted hypotheses, it does lead to certain implications which can be tested experimentally. One of these implications is that if the values of the cue stimuli are reversed, *i.e.*, the positive stimulus made negative and vice versa, *before* the animal begins to show any learning whatever, it should not necessarily make

for any slower learning of the reversed problem, for, according to this theory the animal selects and concentrates, in turn, on certain aspects of the experimental situation as offering possibilities of providing a solution and does not react to the real cue aspect until just at or just preceding the time of solution. Certainly the statement by Lashley that "the practice preceding and the errors following are irrelevant to the actual formation of the association" very definitely supports such an interpretation.

The implication of our own hypothesis for such an experiment, on the other hand, is quite definitely opposite in nature. Thus, if we begin with S_t and S_e equal in strength, or approximately so, and provide training as in the first ten trials of our theoretical example in Table II, such that the excitatory value of S_e becomes greater than that of S_t, but not sufficiently so as to result in the animal reacting to it more often than chance, a reversal of the positive and negative relationship of the stimulus cues will obviously require a greater number of trials for S_t to become sufficiently stronger than S_e to insure a persistent response to it. Whereas in our hypothetical example the last error was made on trial 17 the final error with the reversal of stimulus cues would not be until trial 55.

Fortunately, experimental evidence that makes possible a comparison of the validity of these conflicting implications has recently become available. McCulloch and Pratt (14), in a study of the pre-solution period in weight discrimination by white rats, carried out an experiment that closely approaches the above conditions. The purpose of this particular part of their experiment was to determine whether the assumption "that repeated trials work in a cumulative manner to produce a change in the animal which is necessary for discrimination" (14, 271), or Lashley's interpretation, quoted above, is correct. The issue is essentially the same as presented here, the present theory being an attempt to indicate the nature of the factors operating to produce the cumulative change in the animal. From the theoretical principles here assumed, the deduction follows that there will be such a cumulative change which leads ultimately to discrimination.

The results these experimenters obtained support the deduction of the present theory and are opposed to the implication of the insight hypothesis. The number of errors for the control group, *i.e.*, the one in which there was no reversal of the cue stimuli, was 52.04 ± 3.28, while the experimental group, in which the stimuli were reversed after 28 trials, made 64.35 ± 2.77 errors even after the shift, and approximately 78 errors during the complete period of training. Another experimental group that was given training (84 trials) until they apparently were just beginning to discriminate subsequently made 94.65 errors before learning the converse problem. The authors interpret this result as further evidence favoring the assumption that the learning process represents a cumulative change and furthermore that, at the point at which the animal is just learning a problem, it is not particularly insightful or docile with respect to the relevant stimulus cues. The present writer is now carrying out further studies testing these conflicting theories with chimpanzees serving as subjects. This animal, with its highly complex behavior system, might be expected to be a particularly favorable subject from the point of view of the insight hypothesis.

In concluding this discussion the writer would like to anticipate and attempt to answer a form of criticism with which the type of theory presented here almost invariably has to contend. The criticism is that the kinds of concepts employed by it, such as connections or bonds, reinforcement, inhibition, etc., are inadequate to explain the behavioral phenomena characteristic of learning because the learning process, far from being the blind mechanical affair these concepts imply, consists of unified, meaningful, and purposive behavior. Furthermore, the criticism usually continues, learning behavior does not consist of such isolated part or unit responses but is an organized, integrated process involving the development of insight.

Such a criticism of theoretical concepts reveals an unfortunate misunderstanding of the relation of a theory to the facts it is proposed to explain. It is not, as such critics seem

to think, the theoretical concepts themselves and their hypothetical relations (principles) that must necessarily coincide or agree with the facts of experience but it is the logical consequences or deductions that follow from the theories. The test of the adequacy of any theoretical structure is that the logical consequences that flow from it coincide with the events of experience, *i.e.*, the learning behavior of the animal in this instance. The descriptive characteristics of the facts of behavior do not need to be found in the basic theoretical concepts and principles and, conversely, the failure to find anything in learning behavior descriptively resembling the theoretical concepts is no disproof of their adequacy as a scientific explanation. Finally the question as to whether the theoretical concepts do or do not correspond with reality is a problem not for science but for philosophy. The scientist can justify such ideal constructs wholly from the pragmatic standpoint that they serve as an aid to the integration and comprehension of the observed phenomena. He is under no obligation to imply nor yet deny the possibility of their correspondence with reality.

VI

In summary, the behavioral phenomena characteristic of discrimination learning, such as the non-random, systematic nature of the pre-solution responses, have been shown to be entirely consistent with a 'trial-and-error' type of learning theory. Employing clearly defined principles of reinforcement and non-reinforcement (inhibition) a hypothetical picture or logically possible account of the nature of discrimination has been developed. This theoretical structure provides a rational account of such phenomena as, for example, position responses and the habit of perseveration-with-success-and-alternation-with-failure, and, unlike the so-called insight-hypothesis, which states that the organism, itself, initiates various responses (termed hypotheses) as attempts at solution of the problem until the correct one is hit upon, permits of an account of the conditions determining the appearance and succession of these various modes of response.

While no attempt has been made in the present article to work out in detail the experimental implications of these theoretical principles the following deductions have been made.

(1) Naive, untrained animals will tend to show a more pronounced tendency towards position habits than experienced animals in the solution of discrimination problems.

(2) The response pattern of perseverating on the side which is correct and of shifting to the other on failure will tend to predominate in the experienced animal.

(3) In naive animals that show a long, perfectly consistent position response in the learning period the solution of the problem will occur simultaneously with the abolishment of the position habit.

(4) If the positive and negative relation of the cue stimuli are reversed before the animal is responding to the correct stimulus more often than chance a greater number of trials will be required to learn the reversed problem than would have been necessary for the original problem. A corollary of this deduction is that this difference will be proportional to the time between the beginning of the training and the reversal of the positive and negative stimuli.

(5) Animals will manifest systematic modes of response in an insoluble problem, in which no stimulus is regularly the correct one.

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A RE-EVALUATION OF THE CONCEPTS OF MATURATION AND LEARNING AS APPLIED TO THE EARLY DEVELOPMENT OF BEHAVIOR [1]

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Introduction

In the last few years some definite progress has been made in the scientific understanding of the development of behavior in fetal and newborn animals. In this paper an effort is made to interpret some of these recent advances and to show certain of the broader implications of this new work for the whole study of behavior.

The paper is divided into three parts. In the first section the general nature of the relationship between the organism and the environment is considered. In dealing with this subject, the implications of a modern experimentally established view of Müller's doctrine of the specificity of sensory nerves is discussed in connection with newly discovered facts concerning the development of stimulus-released behavior. In the second section the distinction between the so-called internal and external environments is considered. Here the drives or motives of the organism are briefly discussed in relation to the development of adaptive behavior. In the third and last part of the paper an effort is made, on the basis of the analysis of the two preceding sections, to give a tentative and certainly incomplete suggestion of a functional or operational definition of both maturation and learning, and to evaluate the concepts so defined in connection with observed alterations of behavior in time.

[1] Read as part of a symposium on 'Maturation and Learning' at the Saint Louis meeting of the American Association for the Advancement of Science, December 31, 1935. The paper as presented is given here, save for minor corrections and the addition of a few explanatory and bibliographical footnotes.

PART I

The relationship between the organism and its environment with reference to the development of the special mechanisms upon which the behavior released by stimulation depends.

In developing a consistent point of view in regard to change of behavior in time, it is necessary to define carefully the functional relationships that are being considered. A first step in this process of definition involves a consideration of known facts of the basic relationship between the organism and the energies which surround it. Fundamentally, from this standpoint, a living animal, be it man or amoeba, may be described as an organized system of energies maintaining itself at a certain fluctuating but on the whole balanced rate of living in an external and internal physicochemical environment. These environmental energies are also continually changing, but changing, if the organism which is being acted upon by them is to survive, within rather rigidly defined limits. The range of external and internal temperatures in which the mammal may live exemplifies such limitations. Organisms, however, are not only characterized by self-maintaining activities, but also, at least in certain periods in their life cycles, by processes which are summarized as *growth*. The word *growth*, from this point of view, is used as a compilation-name for the elaborate processes by means of which new and in certain respects more complex organic systems and sub-systems are evolved. Such new systems display truly emergent [2] properties, when they are acted upon by energies external to them.

The human fertilized germ cell is a characteristic living organism. The alterations in structure and function which take place as this simple organism gradually becomes a relatively free adult intelligent human being may be regarded as subsumed under a description of changes taking place in a living system and its sub-systems in relation to a non-static

[2] The word 'emergence' has been used in so many illegitimate ways by philosophers that possibly its use must be abandoned. In the present setting it is merely used to describe the fact that the properties of complex systems are functions of such systems in their observed relationships and not *potential* in the elements out of which the complex systems are synthesized (18, 31–35).

environment. At no point in such a development, of course, is it possible to consider the organism as independent of the radiant, vibratory, chemical and other energies, internal and external, in which it is at the moment maintaining itself and developing. If an environmental energy is altered, a change in the life processes of the organism or of some system of the organism may or may not occur. The details of this relationship can not be generalized, but must be discovered by specific experiment. For example, inner energy relationships between cells and cell groups of the sort summarized in Spemann's concept of the *organizer* (**50**, **51**) may at times during growth persist and run a developmental course, for a period, apparently little influenced by even quite marked external energy changes.[3] Facts of this sort are summarized in Gesell's often quoted statement that "Growth tends toward an optimum realization" (**28**, 230). On the other hand, at certain developmental stages very slight energy changes external to a living system are effective in bringing about marked alterations in that system and in the growing organism as a whole.

It is fundamental to keep in view, nevertheless, especially in dealing with adaptive processes which are alleged to result from maturation or learning, that the significant relationship between the organism and its environment is never one of mere energy transmission. Events observed to occur in the adult mammalian organism itself, which are often loosely said to be caused by external changes, are, strictly speaking, only observed cases of invariable temporal concomitance or temporal succession. In man, for example, radiant energy falls on the retina and after an elaborate series of processes nerve impulses are initiated, or better, possibly, the rate of metabolism in certain neurons is changed. Today it seems more than ever sure that the characteristics of such nerve impulses, save in their temporal aspects, are to be viewed as a function of the specific anatomical neural systems or

[3] For a review presenting one point of view in regard to the complicated problem of the mechanics of embryonic organization and for a current bibliography of such workers in this field as Child, Harrison, Mangold, Spemann, see Weiss (61). For a slightly different interpretation, see Huxley and De Beer (34).

mechanisms so activated (30, 787–793). It is true in many cases that one particular mechanism, and not some other, was set off because it was specialized in such a way that its threshold of excitation was low for the energy thus acting. However, evidence seems to suggest that the characteristics of the impulse in the neuron, once its propagation has been initiated, are characteristics of the neural mechanisms in question and not of the stimulus. Thus, save in time relationships, impulses in neurons are not 'caused by' the stimulus. That is, *the time* of the presentation of the stimulus, and, because of the relative refractory phase, *the intensity* of the stimulus, are both determiners of the *time functions* of discharge of the activated mechanism and, it may be, only of the time functions (1, 56–60). This statement implies an acceptance of a modern form of Müller's doctrine of the specific energy of sensory nerves, both with reference to what has been called modality and quality.[4] But this is not difficult. The work of the present decade in vision (30) and audition (63), as well as in other sense fields,[5] together with the extensive body of knowledge clustering about the all-or-none law of the activity of the neurons, seems to the writer to make such acceptance virtually mandatory. That is, to use the old phrase, the development of 'our knowledge of external things,' and behavior change associated with such development, is dependent on the fact that certain receptor mechanisms are specialized to be stimulated by one sort and only one sort of energy. Thus, of course, the 'properties' of external things, still to use pre-scientific language, are never transmitted over nerves, but merely arouse specific discharges at specific rates in specific neurons at specific times.

From this point of view, the development of adaptive behavior associated with receptor stimulation in ontogeny becomes the account of the gradual evolution and change in the organism of anatomically specialized receptor-neuromotor systems of such a sort that their so-called resting

[4] For a history and evaluation of this doctrine, see Boring (9, 77–94; 10, 127–186).

[5] Nafe, one of the few modern critics of certain aspects of Müller's doctrine, does not criticize the view that in a single nerve fiber there are only two variables: frequency and time (45, 1039).

activity may easily be affected only by very definite changes of a limited kind in the external world of stimuli. By means of inner development, commonly called maturation, mechanisms are thus provided, in organisms that are to survive, which often as a sudden emergence at some point in the growth process, make possible a new means—that of external stimulation—of initiating activity in the cell complexes of the rest of the response mechanism. In this way the processes of growth called maturation produce a receptor-adjustor-effector mechanism of a sort which can be activated and thus further modified as a result of behavior released by external stimuli.[6]

The science of functional embryology or developmental mechanics is devoted to an investigation of the internal energy relationships and morphological alterations involved in the progressive changes concerned in the growth or maturation of the total organism and its sub-systems. Workers in this field have been able to show in great detail some of the factors concerned in the whole series of alterations which take place in the development of the organs of the embryo. The work of Hooker (**32**), Detwiler (**24, 25, 26, 27**) and other experimental embryologists has shown that in a developing organ, such as a limb or a segment of the spinal cord, certain aspects of growth at certain times and in certain degrees are a function of the cells concerned and at certain times and in certain degrees a function of the place in the organism in which these cells are developing. Changes of this sort

[6] A series of experiments previously reported by the present writer bear directly on this point. In these experiments larval amphibia of an experimental group were allowed to develop morphologically in a drugged condition which made external response impossible. After similar organisms of the control group had been free-swimming for some time, the drugged larvæ were removed from the chloretone. In a time no longer than that required for the passing of the release from the drug in another set of control organisms the previously immobile organisms began to move and in a few minutes were swimming as well as the control animals. The control animals, on the other hand, had required some time to develop the perfection of swimming which the previously drugged but morphologically more mature animals acquired almost at once. It has, however, proved impossible in the experiments so far conducted to have the animals develop normally if placed in the chloretone solution too early or if allowed to stay in it too long. This suggests that much of a definite factual nature is still to be learned concerning the independence and the mutual dependence of processes occurring in time in the organism and those initiated by external energy changes (**14, 15, 16, 17**).

involve such activities as cell division, cell specialization, cell migration, and the organization and differentiation of cell groups into new complex systems with new properties, such as those which are called 'dominance' and 'control.' At first the activities of the young organism are essentially intra-cellular and cellular (61). Later the organism more obviously reacts upon its environment.

In this paper it will be impossible even to point to significant new developments in this field as a whole, but it will be valuable, because of their importance in the study of behavior, to consider briefly some of the facts known concerning the developmental stages of effector mechanisms. The first true effector act performed by a cell complex of the organism is not of the sort ordinarily characterized as behavior. In the human fetus before the third week of life primitive heart cells have begun to beat (47). This early rhythmic movement involves a temporal pattern of relatively violent activity in at least a loosely organized or integrated series of cells which are themselves in energetic functional relationship with each other and with other cell systems of the embryo. The complete physiology of cell groups showing such so-called 'spontaneous rhythmic activity,' is still a challenge to the biophysicist. It seems to be agreed, however, that such rhythmic responses involve the summation of chemical processes and depend not only on intrinsic but also extrinsic energy relationships.

Many changes occur in the embryo after the first rhythmic beat of heart cells before the cells of the growing skeletal muscles, the typical effectors involved in behavior acts, reach a point at which they may be activated in such a manner as to contract and bring about complete or partial re-orientation of the organism in space. In the laboratory the first contraction of skeletal muscle cells, as a result of external energy change, follows the direct application of a stimulus to the developing cells themselves. An electrical stimulus applied directly to a muscle will, on and after a certain period, elicit activity of the sort basic to all that will later be called *behavior*. Such responses induced in muscles by direct stimula-

tion are commonly called *myogenic*.[7] The term myogenic used in this sense differentiates such responses from *neurogenic* responses, which, in turn, may be described as responses resulting from antecedent neural activity. Whether or not independent myogenic responses temporally precede neurogenic responses in the normal growth of behavior in the human fetus is not at present surely known.[8] In any case, soon after this myogenic responsive capacity has arisen, reactions begin to appear which depend on antecedent activities in efferent nerves.

Here also there is as yet no final answer to the question as to whether independent central and efferent neural action of the sort just described occurs in the normal fetus at a developmental period before, concomitant with, or after activity has begun which involves the total neural arc. It is certain, however, that before the end of the first half of the gestation period typical skeletal muscle effectors of the human fetus respond as a result of chains of processes involving external stimuli, receptors, and afferent, central and efferent neurons. Such responses can, of course, be shown to be temporally correlated with energy changes activating specific receptor systems of the developing organism. From the very first, however, there is no evidence against the view that such receptor-released responses are characteristics of the receptor mechanism and not of the stimulus energy, save in temporal characteristics, as just suggested.

The first specialized exteroceptive mechanisms in the human and mammalian fetus which can be acted upon by external energies in such a way as to initiate processes which lead to effector action are probably pressure and pain receptors in the skin (19). Before such exteroceptive mechanisms are effective, however, experiment seems to show that proprioceptive stimulation has in certain organisms become capable of releasing effector responses (22, 21). There is also

[7] The problem of the part played by myogenic responses in the development of a functional reflex arc is a complex one. For an evaluation of the work of Wintrebert (68, 69) upon the myogenic stage of development, see Hooker (31, 33).

[8] Minkowski (43) on theoretical grounds believes that such a purely muscular phase of response may exist, but he has not directly observed it in normal human fetal material.

some direct and some indirect evidence that at an early period receptors of the group sometimes characterized as interoceptors also become active.[9] The significance of the distinction between the specific behavior characteristically released by external stimulation and the more general behavior which follows internal stimulation will be considered below. Now, however, in summary of what has been said so far, it may be pointed out that at an early time in the gestation period the behavior of the fetus comes to be changed in specific and systematized ways by alterations in the energies of the so-called external and the so-called internal environments which are at the time impinging upon its developing but always in a measure specialized receptor mechanisms. In order to understand the change in behavior in fetal and neonatal life, therefore, one must remember that not only are internal and external stimulus energies changing, but total specialized response mechanisms including receptors, central and effector organs are also changing. Essential elements of the total arcs are one by one becoming effective and also changing in effectiveness, as a result of intrinsically initiated or 'maturation' changes, and at a later period by changes brought about by previous stimulus-initiated activity.

Part II

The contrast between responses released by the internal and the external environments in relation to the determination of diffuse behavior and specific responses.

The terms '*ex*ternal environment' and '*in*ternal environment' have already been used in this paper. The external environment may be defined as the sum total of all the physico-chemical energies releasing neural activity as a result of action on exteroceptors. The definition of the internal environment is much less simple because it includes certain factors which in other functional settings are correctly enough defined as aspects of the organism and not of the

[9] Yanase (70, 71) has shown that peristalsis, and hence motor movements of a sort calculated to stimulate interoceptors, begin at an early period in human fetuses. Carlson and Ginsburg (13) also show that hunger contractions are present in animals at birth.

environment at all. It is customary to divide the receptors which are internal to the body into two classes, proprioceptors and interoceptors. The receptors included in both of these classifications have, in common with all receptors, the characteristic that they are activated by energy changes external to them. Thus, the sum of energy changes acting on interoceptors and proprioceptors, even though they involve deforming pressures or chemical or electrical stimulation brought about by the body's own active movement, are considered as internal environment. Beside these changes internal to the body, which are, nevertheless, external to interoceptors and proprioceptors, the term internal environment is also often used to characterize the physical and chemical properties of the body fluids.[10] Thus, slight changes in temperature, water content, oxygen, carbon dioxide, carbonydrates,[11] or specialized hormones, etc., in the blood stream,[12] by direct action on receptors, effectors, or central nervous system may directly alter the adaptive behavior of the total organism. In this connection Sir Joseph Barcroft has recently given new meaning to Claude Bernard's famous dictum that *la fixité du milieu intérieur est la condition de la vie libre*. He has shown that both phylogenetically and ontogenetically the complex and specific regulatory mechanisms which maintain this relative constancy of the inner environment in a more widely fluctuating external environment has had a definite developmental course. Fetal blood and adult blood are not the same in oxygen content (6) and the higher centers of the brain are peculiarly subject to

[10] Modern research in this field has progressed far from Carpenter's (20, 38–110) view that 'thought' is to be regarded as the 'expression of chemical change' in the blood, and yet the reluctance of many physiological psychologists to consider the importance of blood change upon the activity of the response mechanism may possibly be traced to the vitalistic errors which entered psychology borne on the stream of 'Carpenter's blood.'

[11] For a general treatment of recent experimental work in this field consult Barcroft (5).

[12] Stone (54) has summarized and interpreted some of the striking work which he and others have done on cycles of behavior in relation to changes in the humoral make-up of the blood stream. This work shows that changes in the internal environment allow external stimuli to release very different responses, for example, in the changing sex cycle of birds. See also the important summary by Moss of the effect of drugs and internal secretions on animal behavior (44).

changes in the oxygen saturation of the blood. Hence, adjustments mediated by such centers can only be carried out under optimal conditions of the internal environment.[13] Are such blood changes, acting directly on centers in the brain, true stimuli, or do they merely alter the irritability of the center in relation to peripherally aroused reflexes? Physiology can say much about this question, but can give no sure answer to it at present.[14] Tracy (**60**) has even gone so far, however, as to suggest, on the basis of his studies of the toadfish, that there is a continuum from behavior supposed to be elicited by change in the carbon dioxide content of the blood, to the voluntary movements of the adult.[15] Again, before leaving the complex field of inner events which may condition behavior, it may be remarked that the processes basic to the Berger rhythm of electrical brain potentials can not well fail, under certain circumstances at least, to influence peripherally aroused activities[16] (Jasper and Carmichael, **34**).

Experimental work on the development of behavior in young vertebrates shows that there are apparently typical differences in the activity of effector mechanisms depending

[13] It may be significant in this connection to remember the increasing knowledge of the part that chemical or humoral agents play in neural activity (see Dale, 23, and Cannon and Rosenblueth, 12). Starling (52, 949) in this connection points out that chemical stimuli alone lead to correlated activity in such organisms as the sponge. Is neural activity to be considered but a high development of this primitive capacity and hence peculiarly susceptible at certain points, such as synaptic junctures, to chemical change?

[14] The basic thesis of the first part of Barcroft's volume, to which reference has just been made, is that the evolution of regulatory mechanisms which maintain the blood constant within very narrow limits of variability in all of its complex components is fundamental to the effective action of the higher centers of the nervous system. In regard to central stimulation, however, Barcroft (5, 312) says: "Are the chemical and the nervous stimuli to the respiratory centre really, in the same sense, stimuli? or is one (the chemical) an increased lubrication of the machine, whilst the other (the nervous) is an increased effort on the part of the man who operates it? Of what is each stimulus capable in the absence of the other? What is the relation of the two combined to each separately? These are questions which it is not possible to answer with any degree of certainty. . . ."

[15] On the basis of his embryological studies of fetal rats Angulo y González says: ". . . the early development of response to stimulation in albino rat fetuses passes through three stages: myogenic, neuromotor, and sensory motor" (4).

[16] For striking characteristics of the rhythm of the brain in connection with other neural and behavioral phenomena see Adrian and Buytendijk (2) and Adrian (1, 78–94).

upon whether the activity is initiated by the external or the internal environments. The phrase internal environment is here used, as apparently it must be at present, to mean either changes in blood which act directly on the central nervous system or external physical energy alterations acting on the non-exteroceptive sense organs of the individual. Tracy (**60**) and Coghill (**22**, 85) suggest, in view of evidence secured on sub-mammalian forms, that some organisms may be considered as first driven blindly (*i.e.*, without exteroceptive guidance) by internal change through their environments. Secondarily, these same scientists further suggest, the internally aroused activity of the animals comes to be directed by exteroceptive stimulation, the very condition of which is the blind internally initiated activity of the organism. Recent investigations on the early development of behavior in such typical mammalian fetuses as the cat, rat and guinea-pig seem to indicate clearly that this picture, attractive as it is, must be considered as too simple to be a satisfactory generalization applicable to all larval and fetal organisms.[17] Recent work, as well, seems to have demonstrated that precisely describable skeletal muscle responses of the reflex type are elicited by externally applied stimulation in certain mammals at the very onset of behavioral life.

The facts just given and others that can not be summarized here lead the writer tentatively to formulate an hypothesis concerning the early development of behavior which seems to be supported by many facts and apparently able to reconcile a major controversy of the present day concerning the development of behavior, and which has the merit of being open to experimental proof or disproof. Briefly stated, this hypothesis is that internally released behavior shown in early developmental life is different from the exteroceptively released behavior of the same period. The first responses to externally applied stimuli in certain mammalian fetuses are definite reflexes, but there is no evidence that similarly

[17] The experimental work of Windle and his collaborators (**64**, **65**, **66**, **67**) and of others, including the present writer (**11**, **19**), seems clearly to indicate that students of behavior are no longer justified in saying that all specific behavior arises as an individuation out of previously less specific behavior.

discrete responses follow stimulus changes in the internal environment. Indeed, the nervous system associated with internal receptors and what is known about the ways in which this system is ordinarily activated, especially if central chemical activation takes place, points to the fact that 'mass behavior' or 'diffuse' behavior, as it has been called, may be regarded as typically resulting from internal stimulation, when the receptor-neuro-effector mechanisms involved have matured to a point to make such general activity possible. This hypothesis makes it easy to see how the same effector mechanism which may be activated as a relatively isolated reflex by exteroceptive stimulation may at another time be activated so as to form part of a much more general pattern of response when a more general efferent neural activation is released by internal change.

The time and stimulus relationships involved in 'generalized activity' are often most complex and difficult to analyze experimentally. For example, carefully applied, punctiform, external stimulation in a fetus may, and typically does, bring about a precise skeletal-muscle reflex-like response at very early periods of motile life. But this very response may stimulate receptors internal to the organism and then this effector-movement-produced stimulation may in turn lead to more generalized behavior. On the other hand, internal stimulation may lead to apparently random or blind skeletal muscle reactions involving a wide efferent outflow which will move surfaces in which exteroceptors are embedded. These exteroceptors may thus be stimulated and in this way precise discrete reflexes may be secondarily initiated.

Care must be taken in evaluating the hypothesis just proposed to remember that strong stimulation acting on an exteroceptive surface may initiate afferent processes different from those initiated by weak stimulation both so far as frequency of impulse and number of afferent neurons are concerned. Such strong stimulation may lead to a more widely distributed efferent outflow. Shifts of connective pattern in the central nervous system as a result of blood change may also alter the generality of response, but such change is to be attributed to internal stimulation as defined above.

It is, of course, obvious that so-called motivated or 'drive' behavior in its simpler forms is probably to some extent to be identified with responses of the sort just described as typically resulting from changes in the internal environment.[18] It has just been pointed out that internally aroused behavior may secondarily lead to exteroceptive stimulation. In an organism said to be able to learn, such stimuli may come to be important in initiating responses which make possible the so-called consummatory act, that is, the act which temporarily terminates the drive state. From this point of view the exteroceptive physical stimulus energy may become a conditioned stimulus or a reduced-cue stimulus which initiates activity, the final result of which is the removal of the internal 'drive stimuli' or change in the level of maintained tonus of some effector mechanism. Therefore, *satisfaction*, if the word is to be used, follows the successful performance of an act so determined. Mechanistically, satisfaction may thus be accounted for in many learning situations without need to consider it as an essential element of the process.[19]

Part III

A re-definition of the concepts of maturation and learning in connection with observed alteration of behavior in time.

When definite changes in behavior (of the sort just suggested as involving conditioned stimuli or reduced cues) begin to be released in the mammal or man can not be said with assurance. As yet, indeed, it is not even easy to state the

[18] For a consideration of this view see Blatz (8) and the papers cited by him and Stone (55).

[19] To attempt to deal with Thorndike's (56, 57, 58) complex and highly conceptualized system of learning and the criticism and support it has evoked is impossible in a few sentences. The view presented here is also, of course, not a new one, but seems, in the genetic setting given here, to suggest a physiological mechanism by means of which the 'effect' of an act can be shown to be different in different learning situations. It thus gives a clue to the possible way in which some unequivocal relationship between such an effect and a quantitative measure of learning can be worked out in many practical situations. It does not require the attribution of 'capacity for satisfaction' to the neurons themselves (56, 57). The view presented here also seems to suggest that physiologically Skinner (49) is not in every way justified in the dichotomy which he makes between conditioned responses of Type I and Type II, although operationally in terms of the concepts which he uses the distinction is clear.

exact point in ontogeny when activities aroused by receptor stimulation begin to release metabolic processes in the response mechanism which bring about relatively permanent modifications of the system of the sort which have been up to that time dependent on the intrinsic activities summarized as maturation. It is also not known whether such externally determined changes are at first indistinguishable from growth-produced changes or whether they, from the outset, possess special characteristics. It seems most probable, however, that at first receptor-released acts do not make any reduced-cue learning or conditioning of responses possible. The first sure evidence concerning the formation of conditioned responses now points to such a change as taking place in early post-natal life, as the studies of Mateer (41), Krasnogorsky(37, 38), Bekhterev (7), D. P. Marquis (40), Aldrich (3), and of Kasatkin and Levikova (35, 36) indicate.[20] It is not demonstrated that conditioning may not occur in the fetal period. The work of Ray (48) and of some unpublished findings of the writer also suggest this possibility.[21] Surely, however, exteroceptively released behavior begins long before conditioning has been demonstrated. When truly learned or conditioned responses do occur, they may still be described in terms of the organism-environment relationships already discussed in this paper. Indeed, and this point should be emphasized, the only novelty physiologically in this new form of behavior determination, as contrasted with that previously called maturation, consists in the fact that the metabolic and electrical activity basic to the alterations of the response mechanism which are fundamental to what is called *learning* can be shown to be a function of antecedent, measured receptor-released activities of the organism.[22] As

[20] For a review of some of this literature see Wenger and Williams (62).

[21] It has been possible to bring about learned food-taking responses in prenatally delivered guinea-pigs before the age at which they would have been naturally born. By the use of fetal electrocardiographs and by other techniques an effort is now being made to study conditioning in earlier fetuses. In this connection it should be remembered that the guinea-pig is born with an unusually well developed nervous system.

[22] It should be emphasized that the present attempt to deal with the physiological basis of maturation and learning does not claim to add one more guess in regard to what is happening in the neurons of the central nervous system when changes occur which subsequently make altered behavior possible. Lashley (39, 34) has well demon-

already pointed out, during the establishment of such externally determined changes, an inner 'drive' mechanism is often simultaneously at work. The learned response is thus frequently set off by a stimulus that initiates a consummatory response, which then leads to an alteration in internal stimulus conditions or induced states of effector tonus, and thus reduces the general level of induced metabolism in certain systems of the organism. It seems, however, as noted above, that this course of events involving an inner drive state is typical, rather than necessary, for all learning. That is to say, if proper time and intensity factors in external stimulating conditions are arranged, mere exteroceptive stimulation may lead to specific internal organism activity and consequent alteration (*i.e.*, central-nervous-system change) and hence to altered behavior at a later time. Here the externally applied stimulus is, as it were, its own drive when releasing well organized receptor-neuro-effector systems of the sort which we have seen can be set off by appropriate, localized, exteroceptive stimulation, even at the very onset of fetal behavioral life. Some of the classical work on conditioning has apparently depended largely on the activation of these early established, inborn, or, as Pavlov called them, 'species' reflexes (46, 25).

Certainly in normal developmental life, nevertheless, it is more common to have exteroceptively determined modifications of behavior arise in connection with, for example, the food-securing activities of the organism than in situations less biologically adaptive. But this does not necessarily mean that the 'two sorts of learning' are physiologically distinct, so far as the central-nervous-system changes in both are concerned.

In view of the discussion just presented, it seems that the word *learning* may be used, in contrast to the so-called growth

strated the fact that at present the picture of cerebral activities "lacks the precision which was anticipated by earlier workers." The present writer believes that a quantitative and functional view of the physiological psychology of learning will show that demonstrated fact is advancing in this field, but that it is still far from an ultimate description in terms of ions, as suggested by Cason (21). This does not, of course, mean that learning can not be given a rigorous scientific description. The relationship between temperature, volume, and pressure of a gas can be stated, even though the locus of each molecule of the gas can not be given.

or *maturation* of behavior, to characterize the development of responses which can be demonstrated to be changed because of certain antecedent stimulus-released processes. In the growth or maturation of behavior, on the other hand, the modification of the organism in time is a function of intrinsic or endogenous processes which, though not occurring in independence of external circumstances, are less directly related to specific antecedent external changes. An example of such an analysis of behavior may be given in brief form in the following three steps: First, the organism's capacity to respond to certain patterns of stimuli, A and B, when presented alone, is measured. Second, when a change in behavior occurs after simultaneous or nearly simultaneous stimulation by A and B for one or more times, the behavior released when A alone is presented is also measured. Third, when the behavior released by A can be shown by experimental variation of various sorts to be truly a function of previous stimulation, and when it could not be previously released by A alone, the measured change in behavior may be said to be learned.[23] When behavior change is shown that is relatively independent of such stimulation it may be said to result from maturation.

Once externally initiated processes begin in the organism, there is no reason to believe that changes of the sort summarized by the word growth stop. Thus, after conditioned responses can surely be demonstrated in a baby, there is ample evidence from Minkowski (43), Tilney (59) and others that the continued development of the cells of the brain may make the physiological process of conditioning at subsequent

[23] The point to be emphasized here, as it is also emphasized in certain of the *Gestalt* theories of learning, is that whatever responses are released in an organism at any given time are a function of that organism as it is at that time and the total of present stimulating conditions. By experimental variation it can be shown that sometimes behavior change is a function of known stimulus-released behavior and sometimes no such relationship can be demonstrated. The statement given above, it will be recognized, does not purport to be a complete operational definition, nor is it a theory of learning. It is merely presented as a sample of a way of dealing with problems of the sort usually considered under the heading of learning without illegitimately mixing assumptions with experimentally determined facts. This is another way of saying: "The principle of conditioning is not an explanation of any instance of learning" as Guthrie points out in his important recent book (29). For a more fully worked out operational theory of learning see McGeoch (42).

times neurologically different than it was when it was first demonstrated. Kasatkin and Levikova say (35) that in the child during the first two or three weeks the cerebral hemispheres are not able to form conditioned reflexes, but the presence of visual differentiation marks the onset of their function. Other evidence in regard to this question has been summarized elsewhere by the writer (18). That the fetal change in the make-up of the blood may also be important in this connection has been shown above.

Therefore, in spite of the fact that the words 'maturation' and 'learning' can be given a valid meaning, physiologically the two processes do not seem to be wholly separable.[24] Wherever possible, therefore, it seems that the scientific study of the change of behavior should be presented in terms of measured stimuli, measured response, and time. Curves so secured and the properties which such curves demonstrate make possible valid generalizations to which such words as 'maturation,' 'learning,' 'fatigue,' 'adaptation' and the like add little, if the scientific meaning of the words only is intended. If something other than the scientific meaning is intended by such words, then, with their use, mythology enters psychology.

Summary

This paper has been devoted to an analysis of the relationship between external stimulation and inner growth in the development of behavior, with special reference to the fetal and neonatal period. Because of the way in which the independent system of the organism and its sub-systems develop and are related to internal and external stimulation, it has

[24] In developing operationally defined constructs, "the more careful we are to define our concepts in terms of those things whose *existence* can be verified the less likely it is that we shall ever have to discard a concept completely" (Stevens, 53). Most of what is called *explanation* in science seems to be operational description in terms of scientifically more simple levels of phenomena. Thus, any concepts which are not amenable to such explanation, while possessing a limited validity, are not apt to be permanent and essential parts of the total structure of science. In the study of behavior, then, it seems to the present writer that those concepts which do not make physiological formulation impossible and which are amenable to growing physiological knowledge are preferable, other things being equal, to those not so amenable.

been shown that, physiologically, changes resulting from environmentally released activity are not necessarily physiologically unlike those resulting from the so-called inner processes of growth. The possibility of defining *learning* and *maturation* in wholly conceptualized terms as a function of a series of observations in time, however, is suggested. The final proposal of the paper is, nevertheless, that the facts of change in behavior in time can best be represented as empirical curves and that these curves and their properties and not such words as *learning* or *maturation* be used in a description of the measured changes of behavior in time.

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